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OF  
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# THE DUBLIN JOURNAL

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## MEDICAL SCIENCE.

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# THE DUBLIN JOURNAL

OF

## MEDICAL SCIENCE.

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JANUARY 1, 1886.

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### PART I.

### ORIGINAL COMMUNICATIONS.

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ART. I.—*Caries of the Spinal Column, and Abscesses resulting from this Disease, with a Mode of Treatment.* By WILLIAM A. ELLIOTT, Fellow and Member of Council, Royal College of Surgeons, Ireland; Senior Surgeon to the Whitworth Hospital, Drumcondra.

CARIES of the spinal column may be considered as one of the most distressing ailments to which the human frame is liable, and although the importance of this subject was recognised by surgeons of eminence, whose opinions respecting the nature and treatment of this disease have been recorded at an early period in the annals of surgery, yet, as we are aware, it is to Percival Pott we are indebted for his elaborate research, and the peculiarly lucid manner in which he has described the symptoms and pathology of spinal caries—a disease but obscurely understood until the publication of his writings in 1783. So exhaustive were the researches of that eminent surgeon, that to the present period but little pathological evidence has been revealed relative to this disease—a fact which is clearly demonstrated by the numerous morbid specimens in our museums, many of which exhibit identically the same appearance and alteration of structure as those described by Pott.

Notwithstanding the amount of literature which has been published relative to the nature and treatment of spinal caries, I think it desirable we should imitate the examples of our predecessors, and that members of our profession who have devoted special attention



to this disease should continue to contribute the result of their observations, and record the best and most successful means which they have adopted for the treatment of this affection.

It is now more than fifteen years since I made a former communication respecting disease of the vertebral column and its treatment. Therefore, after a lapse of such time, and having acquired more knowledge of the result of my treatment, I feel again induced to make some further observations relative to this subject, and the mode of treatment which I have adopted for the last twenty-five years.

It is difficult to limit the period of life at which this affection may invade the system. I have seen it in patients at different ages—in infants of only a few months old, in childhood, in adolescence, in persons up to forty-five years, and sometimes at more advanced periods of life. It is sometimes difficult in the early stage of spinal caries (particularly in infants) to form a satisfactory diagnosis as to the precise nature of the case. We are merely informed that the infant had been for some time unusually restless at night, and not seeming as well as usual, accompanied with feverish symptoms during some part of the day, and occasionally screaming, as if from pain, on being handled; but the latter symptom being frequently absent, renders our diagnosis more difficult. Therefore, in the incipient stage, and when no positive sign of disease has been revealed, palliative treatment is generally adopted to relieve the most urgent symptoms, from which, if the child derives benefit, it may not again be brought under our observation until the disease has become more developed, and when some abnormal appearance in the outline of the spine has attracted the attention of the person having charge of the patient.

Spinal caries usually occurs either in persons originally of delicate constitution or in those who had suffered from the effects of some former ailment. Yet the greater number of patients who come under our observation are those who partake of the strumous diathesis. In the incipient stage of this disease some of the symptoms are obscure, and render it difficult even for the experienced surgeon to pronounce a decided opinion. But when angular projection becomes developed no further doubt need be entertained respecting the nature of the disease.

It is in strumous persons that we usually find the cancellated structure of the vertebræ the part primarily affected, and most frequently followed by absorption, resulting in posterior angular

projection and formation of abscess, whilst the dense structures of the vertebræ are more exempt from inflammatory action. The intervertebral cartilages (although less frequently) may become the structure primarily affected, and by process of ulceration extend to and cause absorption of the adjacent vertebræ.

Circumstances occasionally occur which render it difficult to ascertain the extent to which the disease may have reached, or the number of vertebræ affected. This I have seen in persons of very delicate constitution, who had been long suffering from this affection, and were obliged to observe strictly the recumbent posture, no distinct angle being formed, but merely a gradual curve of the spine, only sufficient, however, to indicate that some latent diseased action had commenced in the part. I have found the treatment of such cases extremely tedious, and likely to be followed by the formation of abscess.

I have been occasionally consulted respecting patients who, as I was informed, were affected with simple lateral curvature of the spine, and who upon examination presented nearly all the abnormal outlines of the figure which are generally observable in this affection, but on minute examination I was enabled to detect incipient caries of the dorsal vertebræ engaging more the lateral than the anterior portions of the bodies of the vertebræ. Cases of this description may be considered the exception, not the rule, in spinal caries. Any cases of this description, which I have seen, always occurred in the dorsal vertebræ, but the treatment was nearly similar to that which I have adopted under ordinary circumstances. Strips of the emplastrum plumbi, spread upon thick sheepskin, should be placed at the lateral projecting part of the spine, and by wearing a well-fitting leather corset gentle pressure can be made upon the parts, and by perseverance in this mode of treatment the figure may be restored to its natural shape.

It is not unusual to find the spinal column in its entire extent bent considerably forward, whilst only a slight amount of angular projection existed—too trifling, however, to account for so much stooping of the figure. But on the application of a well-fitting corset I have seen those persons at once restored, and without much effort, to the upright posture, whilst the angular projection remained unaltered, plainly demonstrating that the deflection of the spinal column was mainly produced by muscular contraction.

When we reflect upon the structure of the spinal column, we perceive how wonderfully this apparently slender pillar is adapted

for the performance of the various functions which it was destined to perform in the human economy. It possesses lightness, combined with strength, to give support to the entire body. It affords protection for the spinal cord and its membranes, upon the integrity of action of which depend the functions of the various structures whose nervous supply is derived from this source. It possesses elasticity and flexibility which are derived from the intervertebral cartilages, which render facile the various movements of the head and upper extremities. It is also capable of sustaining heavy weights, which must be borne by persons engaged in laborious occupations.

It is providential that the spinal column does not more frequently become the seat of injury or disease. But when its functions become disturbed either by injury or disease, we are aware that it is one of the most difficult parts of the body to restore to healthy action.

In no department of surgery has there existed more discrepancy of opinion than respecting the general treatment of spinal caries. Some past distinguished members of our profession who devoted attention to this subject, with whose writings we are familiar, and some practical surgeons of the present period, have entertained their own particular views respecting the treatment of this affection, and it would be superfluous to enter into the details of all these various opinions. Yet there are certain fundamental principles of treatment which have been acknowledged and approved by all—*i.e.*, rest, and constitutional and hygienic treatment; whereas the use of issues, setons, moxæ, counter-irritants, and mechanical support, although strongly advocated by some, have been objected to by others. But, when carefully examining the writings of various eminent authorities on this subject, we find by far the greatest preponderance of opinion is in favour of the adoption of some form of local counter-irritation, and also the use of such suitable mechanical appliance as will afford support to the diseased portion of the spine.

In the treatment of spinal caries we should remember that this flexible column is composed of a succession of joints, any one or more of which are as liable to attacks of inflammation as any of the joints in the upper and lower extremities of the body, and much of the local treatment which we are in the habit of adopting as the curative means in the latter will be found applicable in the former.

Whatever may have been the exciting cause, we know that the incipient stage of this disease is the critical and important period at which decision and promptitude of action will be required for adopting

and carrying out such means, both constitutionally and locally, as will tend to subdue the inflammatory action and arrest the progress of the disease, which, if left uncontrolled, will tend to favour the process of disorganisation in the osseous, cartilaginous, and ligamentous structures, and, by absorption of the bodies of the vertebræ, angular projection of the spinous processes will be produced, which symptoms may be followed by the formation of abscess and paralysis of the extremities.

The constitutional, local, and hygienic portions of our treatment will be found applicable, whatever portion of the spine may be affected; and important as these indications are, yet they are comparatively simple when contrasted with the difficulty of maintaining the diseased portion of the vertebral column in a quiescent state. This object cannot be achieved by merely directing the patient to be kept in the recumbent, prone, or any other particular posture. We must therefore endeavour, by means of some local support, to retain the diseased portion of the spine as motionless as possible during the various movements which must be unavoidably made by the patient.

The hygienic part of the treatment is of vast importance, and upon it too much care cannot be bestowed. The patient should if possible reside in a healthy situation, and sleep in a large, well-ventilated apartment, and the diet should consist of the most nourishing description. If due care be adopted in carrying out these suggestions they will be found most beneficial in the management of the constitutional, local, and other parts of the treatment.

Constitutional treatment is generally found requisite in persons affected with spinal disease, as the system is almost invariably in an unsatisfactory and, to some extent, debilitated condition, arising either from original delicacy or from effects produced by the existing disease. Therefore some form of tonic treatment should be adopted.

The local treatment should consist of counter-irritation applied at the sides of the affected parts of the spine, and the nature of the counter-irritant must be such as to suit the age of the patient. But the surgeon having charge of the patient, should he wish to adopt this course, must form his own opinion as to what remedies he may consider best suited to the nature of the case.

When it is deemed necessary to adopt some form of mechanical support, no doubt need exist respecting the time it should be had recourse to—the sooner we commence this important part of our

treatment the more benefit the patient will derive from its application. I have witnessed this particularly in infants and in children who, at the commencement of the disease, suffered considerably from local pain. I have also in many instances observed a total subsidence of pain take place in a few hours after the application of a light, firm instrument, so contrived as to afford comfortable support to the spine.

We should, however, avoid the danger of relying too much upon mechanical principles as the chief means in the treatment, or the adoption of any form of appliance which would tend to exclude the affected parts from daily observation, and also the application of such local remedies as may be deemed necessary, and for the purpose of daily ablutions, which are so essential to the health of the patient.

Whatever form of spinal support is adopted, it should be so designed as to suit the age of the patient and the part of the spine which may be affected; it should also admit of being easily applied and removed when requisite. The more simple the construction of the support the greater will be the probability of the patient deriving benefit from its use.

In whatever tissue this disease may have originated, careful examination of the morbid specimens which we have seen plainly exhibits the progress of disorganisation and destruction of parts caused by the extension of this disease. In one remarkable specimen, which I have examined, the bodies of eight of the dorsal vertebræ were extensively diseased, and a considerable portion of the cancellated structure had been absorbed; at the side of each vertebra apertures existed, through which matter had flowed—yet, notwithstanding such an amount of disease, no angular projection had been formed, and the natural contour of the spine was maintained. In another specimen four of the dorsal vertebræ had been diseased, the anterior portions of the bodies of which had been absorbed, causing the formation of two angular projections, one salient posteriorly formed by the projection of the spinous processes; the second formed by the sound portions of the vertebræ having passed backwards, and being in close contact with each other, bony ankylosis was formed, which must have diminished to some extent the calibre of the spinal canal. But at this part a deep groove was formed, which was so rounded off as not only to present a smooth surface, but also space sufficient for the medulla spinalis, with its investing membranes to rest upon, without undue pressure.

I have examined other specimens of this kind, presenting iden-



tically the same appearances with those rounded-off angular projections. To this provision of nature may, I think, in a great measure, be attributed the protection which is afforded to the spinal cord and its membranes when they become stretched over this projecting surface. We frequently see persons who have recovered from this disease, although with a considerable amount of angular projection and diminution of stature, who are nevertheless rendered fully capable of discharging the active duties of life. Should, however, the angular projection present a sharp, uneven surface, as no doubt may sometimes be the case, such condition would most likely excite irritation of the spinal cord and its membranes, causing inflammation of the parts, and tending to expedite the formation of abscess.

When disease becomes developed in the cervical region, particularly in the first and second vertebræ, more pain is experienced than when any other portion of the spine is diseased; this, in a great measure, may be attributed to the weight of the head which it has to sustain during its movements, and the greater amount of motion and flexibility which it possesses.

The most difficult portion of the column to retain motionless is that which extends from the first cervical down to about the sixth dorsal vertebra. This part, therefore, will require a specially designed means of support, so calculated as to prevent, as much as possible, any disturbance of the diseased vertebræ. However, from the sixth dorsal vertebra to the sacro-lumbar articulations a much more simple form of mechanical support will suffice.

The pathological evidences of spinal caries should be our guide, and form the basis of treatment in each stage of this disease. The more advanced the stage the more caution we should observe, especially when absorption of the vertebræ has taken place. It is at this period we should endeavour to retain the parts as motionless as possible, and to keep the upper vertebræ in apposition with the adjoining lower vertebræ, so as to promote osseous union between those parts.

We should strictly abstain from making any attempt at elongation of the spine, either for the purpose of diminishing the angular projection or straightening the figure. Any such procedure might cause separation of the vertebræ, and thus retard if not prevent the process of ankylosis; and whilst zealously endeavouring to correct the figure, the recovery of the patient might at least be rendered more tedious.

When children become affected with this disease it is generally

attended with more or less derangement of the system such as to indicate the necessity for adopting some form of constitutional treatment, but more especially when we have reason to suspect the patient to be of the strumous diathesis. In such cases I usually prescribe the following:—

R. Syrupi ferri iodidi, ʒ ij.

Sulphatis quininae, gr. 2.

A teaspoonful to be taken in a little water, twice daily; and either a teaspoonful or a dessertspoonful of cod-liver oil to be given, at night, in milk. I prefer the oil to be given at night, as at this time it will be found to agree better, and not interfere with the ordinary meals, which should be of light and nutritious character.

But should the patient complain of pain in the affected part of the spine, together with constitutional symptoms, the treatment of the case will be rendered much more critical. Our efforts should then be first directed to relieve the pain as speedily as possible. I have seen much benefit derived from the early application of a small leech to the affected part, and it will be advisable to pursue this course prior to adopting any other form of local treatment. Local applications of a stimulating description form an important part of our treatment in the incipient stage of spinal caries. I have used the tincture of iodine and stimulating liniments of various kinds, but generally found them produce pain and uneasiness, particularly in children. The means from which I have seen most benefit derived, and least inconvenience caused to the patient, was the application of small blisters, about one inch and a half long by one inch wide, with an adhesive margin; the blistering surface should be covered with thin tissue paper to subdue the action of the blisters, one of which should be applied at each side of the spinous processes of the affected part, and left on for such time as to admit of the skin being merely reddened—the object in applying the blisters being to determine the blood to the surface of the skin without producing vesication, the occurrence of which may be avoided by having the blisters applied in the morning, as at this time their action can be more carefully observed, and they can be removed at the proper moment.

Should the skin become tender, the treatment may be suspended for a few days, when it may again be resumed, and continued for such length of time as may be deemed necessary. If the mode of treatment which I have suggested be carefully attended to, the same process may be adopted every third or fourth day without producing either pain or annoyance to the patient.

Abscesses connected with caries of the spine are usually tedious in their formation, and may remain dormant for a considerable length of time, eluding the possibility of detection until by their increasing growth they may reach near the surface either at the affected part or, by taking a more tortuous course, may arrive at some remote part, when fluctuation can be easily felt. Much care will then be required upon the part of the surgeon in the treatment of these abscesses, especially when formed in connection with lesion of the atlas and axis. More dangerous consequences are to be apprehended when occurring in this situation than any other part of the spinal column.

We usually find abscesses more rapidly formed in the cervical region, which may, in some degree, be attributed to the more superficial covering of the parts and the great amount of mobility which this region possesses; when matter is formed in connection with disease of the cervical vertebræ the abscess may become prominent either at the back of the pharynx or, by taking its course along the muscles, may present at the side of the neck.

The operation which I have always adopted when tapping psoas abscesses was that recommended by Abernethy. When the abscess becomes fully developed and fluctuation is easily to be felt, I select the parts where the coverings are thinnest, and before any discoloration of the skin has taken place. The skin should be well drawn to one side by the assistant, and avoiding any vein that may appear upon the surface. I then pass a broad, strong, and sharp lancet obliquely through the sac of the abscess, pressure of the hand being steadily continued during the flow of the matter. Should any curdy substance block the opening, I pass a silver director into the sac, and by holding it in a position perpendicular to the wound these substances are enabled to pass through the opening freely, when the matter is again allowed to flow in an uninterrupted stream, pressure of the hand being continued during the entire time, and when the matter has nearly, but not entirely, ceased to flow, I then withdraw the director. The skin, being set free, will form a perfect covering over the wound in the sac, and thus prevent the possibility of air entering. The surface of the part should then be cleaned, and dry lint placed over the wound, which can be kept *in situ* by a broad strip of adhesive plaster. A moderately thick pad of lint of sufficient size to cover the entire surface occupied by the abscess should be then applied. I then pass a bandage from above the knee, extending up the thigh and round the pelvis, for the

purpose of keeping the sides of the sac as closely in apposition as possible.

After three or four days I remove the dressings, and I have always found the wound healed. I again apply the large compress and roller as before, which is worn by the patient for some weeks.

These constitute the only means which for many years I have adopted when performing this operation; and having found by experience that it is perfectly safe and the results satisfactory, I have not found it necessary to make any departure from it.

Abscesses connected with diseases of the vertebræ, although slow in their formation, may be attended with much more serious consequences, when it becomes necessary to give exit to the matter, than when acute abscesses form even at a considerable depth from the surface. We are aware that large abscesses symptomatic of diseased vertebræ may be developed, and by the process of absorption, may completely disappear without the intervention of any operation. Therefore, the utmost care will be required upon the part of the surgeon to decide not alone as to the expediency, but also as to the proper time for operating.

The following cases which I have given respecting some of the sequelæ of spinal caries I hope may be considered of some practical interest:—

CASE I.—I was consulted respecting Mr. T. M., aged about seventeen years. This gentleman resided in a remote part of the country, and was always actively engaged, until his health unexpectedly became impaired. He complained of no pain whatever, but felt extreme lassitude, with loss of appetite, which obliged him to give up his usual occupation. Having ascertained that his internal organs were in a perfectly healthy condition, I then examined his spine, which presented a well-defined incurvation of the spinous processes of the seventh and eighth dorsal vertebræ, which symptoms revealed the nature of the case. I ordered stimulating applications to the affected part, and constitutional treatment which consisted of tonics. I also recommended the use of a well-fitting corset to be worn, which extended from the axillæ downwards over each crista ilii. I also gave directions that the patient should be kept in the recumbent posture, on a firm hair mattress. This treatment was strictly observed for about three months, at the expiration of which time his health became so much improved as to enable him to walk about, which he was able to do with much comfort for about four months. But, having exerted himself too much, the former symptoms of debility returned, accompanied with weakness of the lower limbs, which gradually increased until they became quite paralysed.

I visited this gentleman again, and felt surprised to find his health vastly improved, yet without having gained any power whatever over the limbs, which remained in a cold state, accompanied with numbness. I continued the tonic treatment as before, with the addition of small doses of strychnine—hot mustard stupes to be applied to the limbs, and counter-irritation again to be applied to the affected part of the spine, and the use of the corset to be continued as usual. This mode of treatment was continued until the action of the strychnine became evident. The medicine was then discontinued for some days; and when the twitches and starting of the limbs diminished, the medicine was again resumed, and continued for nearly six weeks, when sensation gradually returned in the limbs, accompanied with increased motor power. The result of this treatment was most satisfactory. The patient gradually regained the use of his limbs, and was able to walk with the aid of crutches. He recovered, and was able to resume his usual avocations.

This gentleman again called upon me after the lapse of three years, when I examined him, and found that no alteration had taken place in the curved part of the spine. He seemed quite healthy and strong, and nothing abnormal in his gait of walking. The nature of his business necessitated his driving long distances; and, when visiting Dublin, he usually called upon me for the purpose of enabling me to see the improved condition of his health and strength. But, notwithstanding my repeated remonstrances as to the danger he was incurring, he still persisted in this course for about three years and a half, when he again consulted me respecting a swelling which had formed in the upper part of his right thigh, but had not interfered with his usual duties. Upon examination I ascertained that a large psoas abscess had formed, which was tense, and fluctuation more distinctly felt at the inner, than any other part of the thigh. I recommended an immediate operation for the purpose of giving exit to the matter, so as to relieve the tension of the abscess. The following day I met the late Surgeon Adams in consultation, whose opinion coincided with that already given by me, so that I at once performed the operation according to the method recommended by Abernethy. Having drawn off about sixteen ounces of apparently healthy matter, I closed the wound, over which I placed a small piece of dry lint, which was kept *in situ* by means of adhesive plaster; a bandage was then applied round the thigh, so as to keep the parietes of the sac as closely as possible in apposition. The patient was kept in bed for five or six days, when I removed the dressing, and, finding the wound perfectly healed, I placed a pad of soft lint over the entire surface, corresponding to the situation of the abscess. I afterwards applied a bandage, which extended from above the knee up along the thigh, passing it over the groin and round the pelvis. The patient was then kept in the recumbent posture for a week before allowing him to move about. In a fortnight after the



operation he returned to the country, with directions to have the bandage and compress applied daily for some weeks.

Six months after the operation Mr. M. again consulted me, and informed me that he had been able to attend to business as before, and without much inconvenience; but, nevertheless, the abscess had again gradually reappeared. Finding upon examination that his statement was correct, I again had the advice and assistance of Surgeon Adams, and, for the second time, I performed the same operation, and observed the subsequent treatment as before—the quantity of matter drawn off being less in quantity but equally healthy as on the former occasion.

I was obliged to perform the same operation upon this gentleman five times, at intervals of about six months between each. Upon each tapping the quantity of matter discharged became diminished, and the quality continued healthy, the consistence being somewhat thicker. Notwithstanding this long-continued and profuse drain, his constitution did not seem impaired, and he was always able to attend to his business.

On the sixth and last occasion of my seeing him, I met Surgeon Adams again in consultation, when the same opinions were pronounced, and the time fixed for the operation. But, being recommended to obtain further advice, he consented to the suggestion, when a different form of operation was proposed. The patient then left town, and afterwards underwent some operation, of the nature of which I was not informed, but which was followed by inflammation, hæmorrhage, and other serious symptoms; and, after lingering for some weeks, death was the result.

CASE II.—J. G., aged thirty-four years, was admitted into the Whitworth Hospital, Drumcondra, on the 26th of June, 1885. From this man's appearance he seemed as if suffering from some severe illness; his countenance was pallid, and his body and limbs were emaciated, but there was no evidence of disease either in the heart or in the lungs. He complained of dull pain in the spine, corresponding to the three last dorsal and two upper lumbar vertebræ, in which situations he felt no pain on pressure. He also complained of pain in the right groin, and he was unable to stand in the erect posture, and any effort to do so produced pain along the inside of the right thigh, which was slightly flexed upon the pelvis; when he attempted to walk his body became inclined to the right side. The symptoms were indicative of disease of the vertebræ, but there was no projection whatever of the spinous processes; any attempt to stand upright produced spastic contraction of the dorsal and lumbar muscles at the right side.

I examined the patient carefully upon many occasions, but could find no trace of abscess in any part. I put him upon tonic treatment—iron, quinine, &c., combined with good generous diet, wine, &c., when necessary. I also

gave directions for him to be kept in the recumbent posture, and allowed to leave his bed only for a short time each day. Under this treatment his health improved, but still he complained of pain in the groin. I then allowed him to wear a leather corset for about three weeks, which enabled him to stand more upright, and gave him much relief, and also enabled him to move slowly about the ward, which he accomplished with comparative ease. He then began to complain of pain in the right hip-joint, and I ordered him to remain in bed. The pain, however, increased in intensity and extended downwards to his foot, taking the course of the sciatic nerve. Having adopted the necessary treatment, after a week or ten days, these symptoms subsided, and there was no trace whatever of any formation of abscess. He was still kept in bed, and lay either in the recumbent posture or on the affected side—the latter, however, he preferred as being more comfortable. Upon making him sit in the upright posture I detected fluctuation of matter which was deeply seated in the right lumbar region, the outlines of which became more rapidly defined than is usual in such cases; when the abscess had reached nearer the surface, a prominence was formed of about three inches in diameter, but unattended with any discoloration of the skin.

Having acquainted the patient that it would soon be requisite to give exit to the matter, his friends preferred having him removed from the hospital to his own home, which they did contrary to my advice.

Feeling very much interested in this poor man's case, I called to see him in about three weeks after he left the hospital. I found him in a very weak condition, and more emaciated than when I last saw him. He had been passing sleepless nights, suffering from loss of appetite and great thirst; he was also suffering from severe pain, which extended from the lumbar region down to the middle of the thigh; the abscess had become greatly enlarged, tense, and painful; he also complained of a painful swelling in the groin, which was not tense.

The case having become urgent, I suggested the propriety of tapping the abscess, more for the purpose of relieving his suffering than from any ultimate benefit that could be expected from the operation. I tapped the abscess by making a valvular opening into the sac, and drew off a large quantity of rather healthy-looking matter, after which I closed the wound in the usual way. The patient derived great relief after the operation, and in three days afterwards the wound was completely healed. In a few days he again complained of pain in the upper and front part of the thigh, with a considerable enlargement in this situation, caused by the rapid formation of a psoas abscess which became more prominent at the upper and inner part of the thigh. The symptoms being equally urgent now as in those attending the lumbar abscess upon which I had operated, I performed a similar operation, and drew off a large quantity of matter, much of the same quality as that drawn off

from the other abscesses. This operation also afforded relief to the poor man. On the third day after operation I examined the parts and found the wound quite healed.

Some days subsequent to the last operation the matter continued to extend downwards and inwards towards the lesser trochanter, from which I infer that the matter had taken the course of the tendons of the psoas and iliacus muscles. Finding that another large depot of matter had been formed in this situation, and the patient's strength having become almost exhausted, I considered it more prudent not to give exit to the matter, as in all likelihood he might have died *in ipso opere*. The sequel proved that my fears were not groundless, as he died on the 3rd of October, being two days after my last visit to him.

CASE III.—I was consulted respecting Master E. W., in the early part of the year 1870. He was then aged about ten years. His general health seemed good, but I observed an unusual rigidity about the lower part of the back, causing a peculiarity in his gait of walking which attracted my attention, but upon examination of his spine I could detect no abnormal appearance; yet, fearing the existence of some latent disease in the part, I recommended stimulating applications to be applied to the lumbar region, and the use of a light corset to afford support to the spine, and as much rest as possible in the recumbent posture.

After the lapse of a year I again saw this patient, but his health and general appearance did not seem in as satisfactory a condition as before. Upon examination of the spine, a curve had appeared corresponding to the spinous processes of the three last dorsal and first upper lumbar vertebræ. Upon more minute investigation I ascertained that an abscess had formed in the left iliac fossa.

I then met the late Surgeon Adams in consultation, and he having agreed with me respecting the nature of the case, we decided upon discontinuing the local stimulating applications, as matter had formed. We prescribed the syrup of the iodide of iron and quinine, one drachm to be given in a little water twice daily, cod-liver oil at night, continuance of the use of the corset, and rest in the recumbent posture. This treatment was continued for four months, when, upon again examining the patient, I ascertained that another abscess had formed in the right iliac fossa, but with greater rapidity than that at the left side. The patient was ordered to remain still in the recumbent posture. The use of tonic medicines was continued—the corset to be worn by day and by night, and only to be removed for the purpose of ablution.

In this state he remained for three months, during which time the abscesses continued to increase in size, particularly that at the left side; at length both attained such large proportions, that they became closely approximated to each other, and by their mechanical pressure produced

vesical irritation, which symptom, however, was not of long duration, having been relieved by gradual absorption of the abscesses which had commenced, and continued for several months.

The patient's health having become much impaired, but without any increase of the spinal curve, I ordered the tonic treatment to be continued, and also the use of the leather corset, which afforded him comfortable support to his back. He was at length enabled to walk about the garden, and to take short drives about the country. Injudiciously, however, he over-exerted himself by attempting to throw sheaves of corn into a cart, and in the effort he unfortunately strained his back, which at the time produced considerable pain, which lasted for several days after the occurrence. The abscesses having been gradually enlarged as before, I was requested again to visit him. Upon this occasion I met Sir George Porter in consultation, when we decided that in consequence of the enlargement of the abscesses, it would be advisable to discontinue the use of the corset for some time, for the purpose of removing all pressure from the abscesses, and also that the patient should again resume the recumbent posture. These directions were strictly adhered to for a long period of time. He again became enabled to walk and drive about the country, and ultimately recovered. The abscesses became completely absorbed, and no traces whatever of them now exist. But the curve which was formed by projection of the spinous processes increased considerably. This gentleman's recovery has been most satisfactory, and for some years past he has been able to walk and drive about with comfort, although his general physique is not as perfect yet as we could wish.

CASE IV.—I was consulted respecting Miss A. D., aged twelve years. She had been suffering from caries of the spine since she was four years old. Upon examination I ascertained that four of the upper dorsal vertebræ were diseased, and, corresponding to this situation, an obtuse angular projection was formed by the spinous processes of the vertebræ. She had been under the care of the late Surgeon John Hamilton, under whose treatment the progress of the disease seemed to have been arrested. The natural form of the chest was, however, considerably altered. The sternum was prominent, with a depression at each side, formed by the yielding of the cartilaginous portions of the ribs. The general health of the patient was impaired; she complained of general weakness, with the sensation of sinking of the body. I adopted the tonic mode of treatment, and ordered a leather corset for the purpose of affording support to the spine. Under this treatment her health became considerably improved. I had frequent opportunities of seeing this child, and although the angular projection became diminished, a considerable amount of spinal curvature remained.

I was requested to visit this young lady on the 4th of December, 1881. She informed me that on the previous day she had used a copious quantity of cold water for the purpose of washing her head, after which she complained of being chilly, with unpleasant feelings about the head. Upon examination I found she could not bear pressure upon any portion of the scalp. The pain was such as to render it difficult for her even to rest her head upon the pillow. She had feverish symptoms, which in a few days subsided, under the use of diaphoretics, and in ten days she was free from all pain, and able to walk about her room.

In three weeks afterwards (but contrary to my advice) she walked out, and persisted in taking some trifling exercise for about ten days. The weather at the time being extremely cold, and her strength being unequal to her making such efforts, she complained of pain and coldness in her lower limbs, with difficulty in walking.

I was again requested to visit this patient, and found her in a very weak and excitable condition. She had lost all relish for food, and her sleep was disturbed by spasms in her lower limbs. I prescribed the necessary treatment during the day, and sedative draughts at night. For a few days she progressed favourably; but, unfortunately, pain in the scalp again returned with increased severity, and passing along the back of the neck, gradually extended along the course of the spine, down to the sacrum. The pain, however, was much more intensely felt in the diseased portion of the spine than in any other part. From this situation the pain continued its course along the ribs, at each side of the body, and having extended to the intercostal muscles, respiration became so much embarrassed that, in the delicate condition of the patient, her recovery for a time seemed very doubtful. Fortunately, however, she had no cough, and she was able to take a moderate amount of nourishment. The pulse ranged generally from 120 to 130, but was small and compressible.

After a week these symptoms greatly subsided, but were soon followed by a general feeling of stiffness of the entire trunk and lower extremities, which were frequently attacked with spasmodic startings and contractions of the legs, down to the feet and toes. These symptoms were succeeded by diminution of temperature, accompanied with numbness and loss of sensation. When the parts were pinched by the fingers, or pricked with a pin, the patient was quite unconscious of it. The lower extremities at length became completely paralysed, but she retained perfect control over the sphincters.

The mode of treatment which I adopted need not be fully detailed. I directed my attention mainly to supporting the general system by means of good, generous diet, wine, &c. I ordered her mild tonics, which were the only medicines I found necessary to prescribe. Stimulating applications were daily applied along the spine, and strong mustard stupes were applied both morning and evening round the lower limbs, being

afterwards enveloped in warm flannel, which assisted in maintaining their temperature.

For three months and a half this mode of treatment was continued. The general health of the patient having improved, I prescribed the syrup of iron, quinine, and strychnine (Easton), a teaspoonful of which was given twice daily in a little water. I also ordered the same local applications to be continued. This mode of treatment suited the case very well. In a fortnight favourable symptoms presented. The temperature of the limbs gradually increased, with partial return of sensation and muscular power; she was able to stand when supported by her attendant, but (as she described) she was unable to feel the ground, the soles of her feet being still destitute of sensation.

In the beginning of April, being four months from the commencement of this rheumatic attack, Dr. Robert MacDonnell visited this young lady, and concurred in opinion with me respecting the nature of the case, and also as to the internal and local treatment which I had been pursuing, and under which she was manifestly improving. I continued the same treatment, and in less than a week after Dr. MacDonnell's visit, the patient complained of twitching and starting in the muscles of the limbs, which, being produced by the action of the medicine, I reduced the dose to a minimal quantity until the twitches and startings in the limbs had ceased, when I discontinued this treatment. I then ordered the compound tincture of bark, a teaspoonful of which was given in a little water, twice daily. This treatment was pursued for some weeks, and with satisfactory results. Her general health became improved, and she was able to move about well, without any awkwardness in her gait. She then went to the country, and was ultimately restored to her former state of health, and with the perfect use of her limbs.

The mechanical appliance which I have adopted in the treatment of caries of the cervical vertebræ is made of thick leather (which is known as "tanned," but not "curried"), which must, before using, be well soaked in warm water, so as to render it pliable. The leather may then be cut so as to pass well over the shoulders, chest, and back. It may then be made to pass upwards and round the neck, and sufficiently high to allow the occiput and lower jaw to rest upon it. The leather, being soft, will admit of its being easily moulded to the part. A bandage may then be applied carefully over the entire surface of the leather, on the removal of which an accurate mould will be made of the parts; and, when perfectly dry, the instrument should be lined with thick chamois leather, with straps and buckles appended to it to keep it firmly on the patient.

The following advantages may be derived from the use of this instrument:—It affords ample, steady, and comfortable support to the entire cervical region. It can be worn by day and by night, and it will also enable the patient to take moderate walking exercise, and it will obviate the necessity of the patient remaining constantly in the recumbent posture.

This instrument can also be easily made available, when the disease is situated in the upper dorsal vertebræ, by merely attaching to its lower and back part a piece of the same description of leather sufficiently long to pass down to the lower part of the dorsal vertebræ, round the body, and made to lace in front. When the disease is situated in about the central part of the cervical region, I have been in the habit of using simply a leather collar, applied round the neck and buckled at the back, the lower edge of which rested upon the clavicle and upper part of the sternum, the upper edge passing under the lower jaw and round the lower part of the back of the head.

The corset which I recommend to be used in cases of caries in the dorsal or lumbar regions is made of the same description of leather as already described, which should be formed so as to nearly close in the front, and made to extend from the axillæ downwards over each crista ilii, and when well soaked in warm water must then be carefully applied round the body; and, by means of a long bandage passed round so as to embrace the parts firmly, an accurate mould of the body will thus be made. It must then be left on for some time to allow of the leather becoming sufficiently hard, when it may be removed, and then lined with chamois leather, and made to lace up the front.

ART. II.—*Address on the Recent Progress of Obstetric and Gynæcological Medicine.\** By THOMAS MORE MADDEN, President of the Obstetrical Section, Academy of Medicine; Vice-President, British Gynæcological Society; Obstetric Physician, Mater Misericordiæ Hospital; Physician to Hospital for Sick Children; Consulting Obstetrician, National Lying-in Hospital, Dublin, &c.

GENTLEMEN,—My first duty is to express my grateful appreciation of the honour conferred by my election to this Chair. Fully conscious as I am of inability to emulate in aught else those who

\* Delivered in the Obstetrical Section of the Academy of Medicine in Ireland, Friday, November 27, 1885.

have heretofore presided at our meetings, I must only venture to hope that at the expiration of my tenure of office I shall not prove to have discredited your selection by any failure in my effort to discharge the functions temporarily entrusted to me. Fortunately, however, whoever occupies this position can always count confidently on the support of his fellow-members of the Academy, as well as on the good feeling and tolerance evinced in its discussions, which no less than the importance of the subjects debated, and the ability with which they have been generally handled, has so much served to advance the twin sciences of obstetrics and gynæcology here cultivated.

During the past session there have been seven meetings of your Council and seven ordinary meetings of this Section, which were occupied by the exhibition of numerous pathological specimens of great histological interest, and by the reading of the following papers, viz.:—1st, "On Fourteen Cases of Ovariectomy," by Dr. Macan; 2nd, "Cases of suppurating Ovarian Cyst complicated by deeply-seated Pelvic Abscess," by Dr. Atthill; 3rd, "On Hemiplegia after Delivery," by Dr. Neville; 4th, "On the Treatment of Uterine Fibro-myomata," by myself; 5th, "On artificial Vesico-vaginal Fistula," by Dr. Macan; 6th, "On some Points in the Diagnosis of Pelvic Hæmatocele," by Dr. William Smyly; and 7th, "A Memoir on Intestinal Obstruction," by Dr. Purefoy. Of these communications, with one exception—viz., my own paper—it may be said that they were not unworthy of the reputation of the Academy. But though the character of our proceedings has been thus creditable, the number of papers read here was somewhat smaller than it might have been, remembering that there were only seven communications as against ten read in the Medical, twelve in the Surgical, and twenty-four in the Pathological Sections. It is therefore to be trusted that in the coming session there may be an increase in this respect, and that our brethren throughout the country will more frequently favour us with contributions. We are, of course, well aware that in too many instances the time of medical practitioners, especially those in country districts, is so occupied by the almost incessant and ill-requited duties to which they devote their lives, as to leave scant leisure for the preparation of elaborate essays. But even the busiest has some intermission of labour, and how can these *horæ subsecivæ* be better employed than in recording the cases of interest or the practical remarks that must occasionally occur to



every observant man. Such contributions would, I am sure, be gladly welcomed here, and their increased number will best prove that in the fusion of the old Dublin Obstetrical Society with the Academy of Medicine, its spirit has been renewed rather than extinguished. For as from the ashes of the fabled phoenix there sprung a new and more vigorous creation, so from the second birth of our body corporate has arisen this Academy, in the proceedings of which are manifest not only the revived enthusiasm and energy of youth, but also a maturity of experience derived from the older associations, of which it is the fruition.

From the dawn of clinical obstetric teaching, dating from the foundation in 1745 of the Dublin Lying-in Hospital by Bartholomew Moss, down to the present time, our School of Midwifery has ever occupied a prominent position in the van of obstetric science. I therefore can see no reason why we should now sit submissively at the feet of any of those foreign accoucheurs who, having at last learned the value of methods of treatment with which we were long previously conversant, on the strength thereof are regarded as shining lights of obstetric science. Thus the "expression of the placenta," by the method with which the name of Professor Crédé of Leipzig is so generally associated, has been here constantly practised from time immemorial. The operation of version as a substitute for craniotomy, as advocated by recent English and German authorities, was first carried into effect by Sir Fielding Ould, of Dublin, in 1742, and a century subsequently was revived in our Lying-in Hospital by the late Dr. M'Clintock, whose fame should endure as long as genius, erudition, and eloquence, as well as unswerving rectitude and unsurpassed obstetric skill, constitute titles to remembrance in our profession. In like manner, the preventive and the immediate reparative treatment of perinæal lacerations, the revival of the timely employment of the forceps, the prophylaxis of *post partum* hæmorrhage, and countless other improvements in the management of child-birth, the credit of which are elsewhere claimed, have all emanated from this ancient School of Midwifery. Moreover, following, as we do, in the immediate track of a Society in whose Transactions the contributions of Every Kennedy, Montgomery, Churchill, Beatty, Ringland, and, above all, M'Clintock, are recorded, together with those of others no less distinguished, it behoves us to endeavour to imitate the untiring energy, patient research, and accurate clinical observation of which they have set the example.

By so doing, even the humblest who takes part in the work of this Academy may perhaps add something to the common fund of knowledge, and so aid in the advancement of our science and the relief of the sufferings to which it is its object and our privilege to minister.

A brief retrospect of some improvements which have of late years been effected in obstetric practice, and to which the labours of the former Dublin Obstetrical Society have largely contributed, will best exemplify the prospective utility of this Section of the Academy. For example, if we briefly contrast the midwifery practice of the present day with that inculcated and acted on within the memory of those who, like myself, have approached the *mezzo del camino della nostra vita*, we shall observe that within that comparatively short period childbirth, conducted on the principles of modern science, has been largely divested of its former sufferings and dangers. Thus, by the more general employment of anæsthetics, the throes of labour have been rendered endurable. By more timely and judicious use of the forceps and other improved methods of affording assistance in difficult cases of parturition, the duration of that period of former agony has become abridged. By the employment of the means available for anticipating and warding off the occurrence of *post partum* hæmorrhage, as well as by our knowledge of the local value of perchloride of iron, for which we are indebted to Dr. Robert Barnes, as also by the use, in modern practice, of Sangrado's trusted remedy—hot water, as now employed in the treatment of *post partum* flooding—that cause of obstetric mortality has been almost completely removed. At the same time the whilom most frequent and most serious of the dangers consequent on childbirth—viz., puerperal fever—has been strikingly obviated by the prophylactic and antiseptic precautions at present taken for its prevention. Moreover, if, as may still happen, notwithstanding these preventive measures, or where they have been disregarded, that septicæmic puerperal disorder should supervene, we are now armed with rational and effective therapeutic resources for its treatment, and are no longer in the position of helpless spectators of what to a former generation was the irresistible course of disease to its fatal issue. Lastly, by the general substitution of methods of delivery compatible with the safety of both mother and child, the proportion of cases in which embryotomic instruments are employed has been so minimised as to lead to a confident expectation that all

such implements as the perforator and crotchet, cephalotribe or craniotomy forceps, will be relegated from the obstetric armamentarium to the chamber of horrors of some future museum of surgical instruments. At any rate, from the progress recently made in this respect, we have good reason to look forward to a not distant date when the great objects of obstetric science—the delivery of a living child with safety to its mother—may in every instance be still more perfectly realised than even at the present time.

#### DIMINISHING MORTALITY IN RECENT MIDWIFERY PRACTICE.

Now, if we come to examine the results of those improvements in modern midwifery that we have just alluded to by the light of the successive Annual Reports of the Registrar-General, we shall find much reason for congratulation, as well as for hope, that in the still more perfect obstetric practice of the future, the mortality of childbirth and the puerperal state will be yet further diminished. Thus, in the 10 years from 1861 to 1870 the deaths from childbirth and puerperal fever averaged 60 in every 100,000 women living, between the ages of 20 and 55; whilst during the 5 years 1876 to 1880 they fell to 53 in the same number of women between those ages. Not many years have elapsed since it was estimated by Dr. Matthews Duncan that the mortality occurring in connection with childbirth in these countries was something like 1 in 120 women confined. This statement, even regarding it as being a mere approximation, bears out what has been said with regard to the diminution of childbirth mortality within the last twenty years. It has been observed by the present Registrar-General for England that this mortality is best measured by the proportion of mothers who die to the infants born in the course of the year. Therefore, taking this for our standard, we may contrast the mortality thus occasioned in the year last reported on by the Registrar-General—viz., 1883—with Dr. Matthews Duncan's former estimate, and we find that, in 1883, 6,043 women died in England and Wales after childbearing, and, presumably, from causes in connection therewith. This, in proportion to the total number of children born alive, would amount to about 6·1 maternal deaths per 1,000 births, or 1 in 230, instead of 1 in 120. Moreover, with regard to the most important cause of puerperal mortality, we learn from the recently published report of the Irish Registrar-General that the deaths registered for puerperal fever in this country last year had fallen to 300, being 63 under the average for the 10 previous years. The diminution of

mortality in obstetric practice in this city is also observable in comparing the Reports of the Rotunda and Coombe Hospitals twenty years ago with those of the same institutions at the present time. Thus, in the 7 years which ended in 1864 there were 8,224 births in the Rotunda and 252 maternal deaths, or 1 in 32. During those years 3,142 deliveries took place in the Coombe Lying-in-Hospital, and 45, or 1 in 70, of those delivered therein died; whilst last year, according to the Report of the Board of Superintendence, there were 9 deaths in 1,148 deliveries in the Rotunda, or 1 in 122, and 7 deaths in 455 deliveries in the Coombe.

#### MORE FREQUENT EMPLOYMENT OF FORCEPS.

Of all the improvements which have thus tended to the greater safety of parturition, probably one of the most important is the more frequent and judicious use of the forceps in modern practice. I may, therefore, very briefly refer to some statistics, on which I have elsewhere<sup>a</sup> enlarged, in proof not only of the saving of maternal life and suffering which has resulted from the gradual re-introduction of the forceps into common use, but, still more, as showing that an increasing frequency of forceps cases may be regarded as practically synonymous with the desuetude of child-destroying instruments. Formerly the forceps was hardly ever resorted to until the parturient woman, worn out by the protracted sufferings she had endured, was almost moribund, and when, too, the child was probably dead, in consequence of the long-continued pressure it had been subjected to. Thus, twenty-six years ago, Dr. J. Hall Davis, in his work "On Difficult Parturition," informs us that he only found it necessary to use the forceps on seven occasions in 7,371 deliveries, or once in every 1,053 labours. In the statistical Reports of the successive Masters of the Dublin Lying-in-Hospital, we find the most conclusive evidence of the advantage which has followed the more judicious use of the forceps in later years in that institution. During the Mastership of Dr. Joseph Clarke, from 1787 to 1794, there were 10,387 deliveries in the hospital, and the forceps was only applied in 14 of these, with 6 deaths. But the more easily used perforator and crotchet were resorted to in 49 cases, with 15 deaths. And in his private practice, extending over forty years, Dr. Clare only once attempted to use the forceps. In Dr. Labatt's Mastership, from 1815 to 1822, during

<sup>a</sup> Lectures delivered in the Dublin Lying-in-Hospital By T. More Madden, M.D. Second Edition. Dublin. 1879.

which time 21,867 births took place in the hospital, the forceps does not appear to have been used in any instance. From 1826 to 1833 Dr. Collins used the forceps in 24 cases out of a total of 16,654, but employed the perforator in no less than 118 cases, with 24 deaths. From 1842 to 1845 Dr. Charles Johnson used the forceps in 18, the vectis in 16, and the perforator in 54 cases, in 6,702 deliveries. From 1847 to 1854, in Dr. Shekleton's Mastership, there were 13,748 deliveries in the Rotunda, and the forceps was now used in no less than 220 of these, and the perforation in 54. Dr. M'Clintock, who ruled the hospital from 1854 to 1861, brought the forceps into still more frequent requisition, and in his last three years of office employed it, or the vectis, in 76 cases, or once in every 60, in 3,700 deliveries, whilst the number of craniotomy cases was reduced to 5. The succeeding Master, Dr. Denham, was a still more constant advocate for the timely use of the forceps. To Dr. Johnston, the next Master, belongs the credit, however, of having brought the forceps into more frequent use than had ever previously been the case. Thus, from November, 1868, to November, 1874, in 7,027 deliveries the forceps was used in no less than 639 cases, or about once in every 11 cases, with only 39 deaths, while the proportion of craniotomy, or cephalotripsy, cases has been reduced to 29.

The foregoing statistics, as I have already said, unquestionably demonstrate that, as the forceps is used more frequently, the mortality in the cases in which it is employed diminishes; and, secondly, also shows the happy effect of the free use of the forceps in lessening the proportion of craniotomy cases.

#### COMPARATIVE ADVANTAGES OF DIFFERENT FORMS OF FORCEPS, AND METHODS OF EMPLOYMENT.

Amongst the changes which have recently taken place with regard to the forceps there are two, however, which I venture to think require further consideration. The first is with regard to the early period of labour at which instrumental assistance is advocated by some authorities. The second is with regard to the complicated form of forceps introduced by M. Tarnier, and since variously modified, and largely employed by modern obstetricians. For my own part I can see no reason for instrumental assistance before the os uteri is fully dilated, except in certain cases of complex labour, where immediate delivery may be necessary for the safety of mother or child, and in which it must be unhesitatingly resorted to as soon

as the os uteri is sufficiently dilatable. But if obstetric practitioners should ever come to regard it as a safe rule of practice to apply the forceps as soon as the os uteri can be sufficiently expanded to admit its introduction, which in some instances may be done before the occurrence of any true labour pains, is it not probable that the ill results of the indiscriminate and injudicious employment of this practice will outweigh all the possible benefits of its right use? The preference generally given to Tarnier's axis-traction forceps by some British, as well as by nearly all French, obstetricians over instruments such as Barnes' original double-curved, or my own short forceps, appears to me to be a mistaken one. In operative midwifery, as in any mechanical problem, it is obvious that there should be a due proportion between the power used and the resistance to be overcome, and that the force employed should be the minimum necessary to accomplish the desired effect. Now, whatever may be said to the contrary, this is certainly not the case in Tarnier's forceps, which is a needlessly complicated, unwieldy, and, for the purpose for which designed, an ill-contrived piece of mechanism. Hence, in my opinion, this instrument is by no means equal to Dr. Barnes' original forceps for any cases of difficult labour where the head is detained above the pelvic brim; nor, I will venture to add, to my own short forceps in those still more frequent instances in which, after the head has entered the pelvic cavity, assisted delivery may be expedient, as I have found in upwards of 250 cases in which I have now used this instrument.

#### CAUTION NECESSARY IN THE USE OF INTRA-UTERINE INJECTIONS IN THE PUERPERAL STATE.

With regard to the prophylactic use of intra-uterine aseptic injections, now advocated and employed as a rule of treatment during the puerperal state, I may observe that whilst convinced, by my own experience, of the paramount importance of thoroughly washing out the uterine cavity in cases of threatened puerperal septicæmia as the best means of warding off that danger, as well as of the value of this practice in cases where such disease has actually manifested itself, as a most efficacious method of arresting its course, I am no less persuaded of the possible risks of this procedure, which, in my opinion, should never be resorted to without necessity, and should be then carried out, not by the nurse, but by the accoucheur himself. In the course of a tolerably long experience I have had abundant evidence

that intra-uterine injections, when administered by the ordinary syphon syringe, are by no means always as harmless as they are commonly supposed to be. I have elsewhere reported cases of acute metritis, uterine colic, and even of embolism, caused by this much-abused instrument, and hence I now generally recommend uterine irrigation instead of syringing. For this purpose my own irrigator (as described in the third volume of the Dublin Obstetrical Society's Proceedings, p. 183) is probably as good as any other, being portable, easily employed, and readily constructed. In proof of the advisability of greater caution than some may think necessary in the use of the mercurial antiseptic intra-uterine injections, so largely employed by some obstetricians, I may here cite from the *American Journal of Obstetrics* the history, not long since reported by Dr. Partridge, of New York, of "a case of labour that had occurred at the Nursery and Child's Hospital, in which vaginal injections of bichloride of mercury, 1 to 2,000, were used, and the patient did well for three days. On the third day she had a chill, and the house surgeon gave a intra-uterine injection of the same solution. The next day there was another chill, and the injection was repeated. This was followed by bloody passages from the bowels, and death took place. Intense colitis was found *post mortem*. Dr. Partridge referred to reports of three other cases of supposed mercurial poisoning from the same cause. The patient whose case he had related died within sixty hours from the administration of the first intra-uterine douche." At the same meeting of the New York Obstetrical Society at which the last case was referred to, Dr. Partridge also related a case in which, by mistake, a nurse threw a bichloride injection into the bladder instead of into the vagina, and severe cystitis was set up—quite as much, perhaps, from mechanical violence as from any special action of the bichloride.

#### RECENT DEVELOPMENT OF GYNÆCOLOGY.

Turning now to Gynæcology we will find that remarkable as has been the recent progress of obstetric medicine, still more wondrous is the contemporaneous development of gynæcology. So rapid, however, are the strides with which this youngest creation of modern medico-chirurgical science is daily advancing, and so widely extended are the boundaries of the field of practice cultivated by its followers, that it would be unprofitable, in this hurried retrospect of the progress of the work to which our Section in

the Academy is devoted, to do more than merely allude to some few of the many pathological and therapeutic problems of late solved by gynæcologists, or with which they are still occupied.

To exemplify the change that has taken place in this department of medicine, even within the recollection of many here, I shall briefly contrast the faulty diagnosis and unsatisfactory treatment of most utero-ovarian diseases in my own student days with the results at present attainable from the modern science by which the crude gynæcological knowledge of twenty years ago has been so happily replaced.

At that time the interior of the living uterus was still a sealed book—a veritable *terra incognita* to uterine specialists then unprovided with any efficient means for dilating its orifice and exploring its cavity, or with any direct method of dealing with intra-uterine diseases, which, thanks to the procedures since suggested by Kidd, Atthill, and Ringland, may now be easily recognised and effectually treated. In the pre-antiseptic surgical period to which I refer it would have been impossible to foresee the realisation of that triumph of modern surgery which is now exemplified in gynæcological practice by the results of abdominal section for the removal of ovarian tumours, as well as in some other uterine, intra-peritoneal, and pelvic morbid conditions. Although the feasibility of this had been anticipated and demonstrated by Houstoun, of Glasgow, in 1701, and a century later first carried into effect with great success in America by Dr. M'Dowell, of Louisville, as well as, subsequently, by other surgeons—foremost amongst whom were Dr. Clay, of Manchester, and Sir Spencer Wells—still, even twenty years ago these operations were hardly yet generally recognised as legitimate. Indeed, had anyone then ventured to predict that such tumours would ever be removed by any surgeon in consecutive hundreds, and even thousands, of instances, with increasing success, such an anticipation would have been regarded as the day dream of a crack-brained enthusiast. Not merely sub-peritoneal morbid conditions, but also those interstitial and submucous uterine tumours, which are now successfully treated by every gynæcologist, were then practically beyond the range of surgical interposition. In those days, too, the operative treatment of vesico-vaginal fistula, even in ordinary cases, was more generally a failure than successful. And although the proper method of dealing with such cases had been previously pointed out by Sir James Simpson and others, still down to a recent period



teachers as accomplished as the late Dr. Churchill were content to advise mere palliative measures in these cases; whilst in instances of more extensive disruption of the vesico-vaginal walls, patients, who would be now curable by the plastic operations variously modified by Marion Sims, Bozeman, Emmet, Goodell, and other American surgeons, were then abandoned to lives of hopeless misery, to which death alone afforded relief.

Fifteen years ago, when Dr. Graily Hewitt recalled attention to uterine malpositions, and suggested improved means for their treatment, such flexions and displacements were imperfectly differentiated, their importance was unrecognised, and their treatment in many instances was erroneous. The physiology and pathology of menstruation only of late years becoming understood from the researches of Drs. Wiltshire and Williams, the management of its abnormalities was before then largely empirical. The bearing of cervical lacerations on pelvic pathology, as elucidated by Dr. Emmet, was previously ignored. Finally, many of the morbid conditions of the uterine appendages to which, and more especially to those of the Fallopian tubes—viz., salpingitis, pyosalpinx, hydro-salpinx, &c., to which such importance is attached by gynæcologists at the present day, were wholly unknown and untreated a few years ago.

The practical results of the changes that have thus influenced gynæcological opinion and procedure during the last twenty years, is best estimated from the progressive diminution of female mortality within this period, as shown by the Registrar-General's Annual Reports.

#### NECESSITY OF GYNÆCOLOGICAL SPECIALISM.

The prolongation of life, mitigation of suffering, and successful treatment of diseases formerly regarded as incurable and necessarily fatal, which, as just shown, have resulted from the progressive development of modern gynæcology, afford, I think, a more than sufficient answer to all the attacks of which our special branch of practice has been the object. Such accusations against medical men belonging to a class of specialists, most of whom are probably quite as honourable and highly educated as any in the profession, are very regrettable. And as Dr. Clifford Allbutt has chosen, at the last meeting of the British Medical Association, to repeat his attack on our specialism and its followers—save Dr. Playfair, whom he seems to regard as one of the few exceptions to the

general class of gynæcologists—I may also be permitted here to reiterate that which I have before urged in reply to his observations—viz., that it is the veriest waste of the time of that or any other modern medical society to occupy it with declamations against the prevailing tendency to specialism in all branches of the healing art in general, and more especially directed against gynæcology. I would presume to remind our critic that in the present state of medico-chirurgical science it would be impossible for any one to cultivate equally all its component parts. Hence a division of medicine is necessitated, more especially in all large centres of population, in the interests alike of the profession and of the public, by whom it is justly supposed that physicians who confine themselves to a limited field of special practice will probably become more experienced, and be more reliable advisers therein than others can be whose practice extends equally over every branch of medicine, surgery, and obstetrics. Under these circumstances, those who now seek to oppose medical specialism, and to discredit gynæcology in particular, are attempting a task as vain as was Dame Partington's effort to keep back the advance of the Atlantic waves with her poor besom. Nor indeed can they who devote themselves to gynæcological practice be said, with any truth, to be engaged in any narrow specialism. On the contrary, at every turn those thus occupied find abundant evidence of the correlations and inseparable interlacements of peri-uterine affections with disorders of the general health; and these are no less exemplified in the protean varieties of nervous derangement which from the most trivial manifestation of hysteria to the gravest forms of cerebro-nervous disturbance—viz., epilepsy and insanity, so frequently associated with the local diseases that come within the special province of the gynæcologist. It is therefore surely obvious that whoever would successfully pursue this calling can be no mere specialist, but must, as I have already said, be a well-educated physician in the fullest and highest sense of the term, thoroughly conversant with the principles and practice of medico-chirurgical science.

#### UNDUE INFLUENCE OF SYSTEMS ON PRACTICE.

It is not improbable that gynæcological practice might have made still greater advances than even those which have been referred to had not its progress been somewhat retarded by some circumstances incidental to its earlier history. Of these, one of the most important

was the over-hasty generalisation by which successive discoveries and improvements, however important and valuable in certain cases, were dignified into systems, and extended to cases wholly beyond their proper sphere. The undue extension of the theories that within the past few years have mainly influenced gynæcological practice, and all of which point to different forms of exclusively local treatment in gynæcological disorders, has contributed to crowd out of notice the importance of constitutional treatment in conjunction with whatever special local treatment may be necessary.

#### ABDOMINAL SECTION.

I may here allude to the enthusiasm prevailing with regard to abdominal surgery in the treatment of malignant and other diseases of the uterus and its appendages. None can estimate the value of such operations in appropriate cases more than I do. But though I fully recognise the success which has attended the recent practice of some authorities in this branch of surgery, still I would venture to remind my younger hearers that such procedures, however successful they may be in exceptionally skilful hands, are not to be lightly undertaken by others, nor resorted to without absolute necessity, and that some cases in which abdominal sections are recommended might probably be as advantageously dealt with by medical and palliative as by surgical methods. Thus in a paper which I read here last Session, on Uterine Fibro-myomata, I endeavoured to show that in their treatment abdominal section, although in some instances necessary, was by no means invariably indispensable. I pointed out that such tumours might occasionally be removed by enucleation per vaginam; that in other cases they could be kept in check or their symptoms obviated by purely medical means; and that in others, again, they called for no active treatment whatever. In so doing I referred to statistics published by several eminent specialists, one of whom (Mr. Lawson Tait) has since complained of being misrepresented in a passage in which, alluding to certain incomplete operations, I said—"Of these incomplete operations Mr. Lawson Tait thinks that he 'may speak with a certain amount of satisfaction,' though from whence he derives this contentment I am at a loss to understand, as his mortality in them was 50 per cent." He says—"Of one group I think I may speak with a certain amount of satisfaction, that is the group which includes thirty cases of incomplete operations, even if all that I can say is that three per cent. of incomplete

operations is not a large proportion, and that I have the satisfaction of knowing that it is still on the decrease, as my experience grows."

In reply I pointed out to Mr. Tait that in the passage to which he takes exception I was referring to the rate of mortality in one particular group of cases, which was stated to be 50 per cent., and that I saw no unfairness in my not having quoted those words, of the omission of which he complains, as they do not bear on the stated mortality in that group of operations, but referred to the diminution of incomplete operations in his practice, and not to their result. In my paper and elsewhere I have fully acknowledged Mr. Tait's skill and ability, and as I feel free from any consciousness of having misrepresented his statements, I regret that he should be under the mistaken impression that I desired to do so.

#### MEDICAL WOMEN FOR INDIA.

Our profession knows not clime, or creed, or race, in the scope of its beneficent operations; and, therefore, over-long as this address has perhaps already been, I shall in conclusion venture to add a word on a matter which appears to me of concern to all who, as obstetric or gynaecological practitioners, are interested in the advancement of and extension of the sciences which have for their object the prevention and treatment of the sufferings and diseases peculiar to women. I refer, namely, to the recent appeal which has been made by Lady Dufferin, the worthy consort of our distinguished fellow-countryman, the Governor-General of India, in behalf of the efforts now in progress under her auspices, for extending through the agency of duly qualified medical women the incalculable benefits of our branch of the healing art to the millions of Indian and other oriental women, who, by the prejudices of their race and creed, are still doomed to endure the pains and dangers of childbirth, and the protean forms of disease incidental to their sex, without any possibility of relief from skilled medical assistance. If, therefore, so large a portion of our fellow-creatures are thus condemned to parturient, gynaecological and other sufferings and risks from which their sisters in happier Christian countries are now freed by the aid of our science, no higher duty can devolve on us than that of co-operating as we may in this work by taking part in the training and education of those female practitioners who alone can gain admission to the sick chambers of oriental womankind.

Whatever difference of opinion may exist with regard to the expediency of women practising medicine and surgery in this

country, there can be no question as to the necessity of supplying such practitioners for the purpose of attending their own sex in lands where no other skilled aid is admissible. The medical corporations of this city have been amongst the first to admit women to their diplomas, and our Academy has followed their example by receiving within its fellowship, irrespective of sex, all who may be qualified for that honour. No place can afford larger facilities than are here available in our great hospitals for the fitting education of women for this most important work; and I am confident that in the institutions with which so many of us are officially connected, as gynecologists or obstetricians, every possible encouragement and assistance will be afforded to any ladies who may therein seek to qualify themselves for such a mission of mercy to the countless women who, in India and elsewhere, have hitherto been beyond all reach of the ministrations of our noble calling.

It now only remains for me, gentlemen, to thank you again not only for the honour you have done me in placing me in the position of your President, but also for the patience and apparent interest with which you have borne with the observations with which I have occupied your time.

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ART. III.—*Experimental Researches upon Tuberculosis and Scrofula.\**  
By EDWIN WOOTON.

II. SCROFULA.

VERY generally the two pathological terms heading this paper are used as indicating but two different degrees of one condition. It has been often laid down as a medical axiom that scrofula is merely a coarse form of tuberculosis, and it is the common acquiescence in this scientific heresy which has rendered the treatment of the diseases a matter of empiricism, for the existence of such an axiom predicates the grossest ignorance as to their pathology.

Surprise is naturally felt that the *primâ facie* differences in the external phenomena exhibited by the two conditions did not indicate to the earlier observers that there were intrinsically different pathological processes at work in their production. To describe scrofula as but a coarser form of tuberculosis means, if anything, that the pathology of the two states is identical in kind but different in degree.

\* Continued from Volume LXXX. No. 166. Third Series. Page 290. All communications on the subject-matter of these papers should be addressed to me, until further notice, at 10 Westbourne-terrace, North, London, W.

The opinion to which I have alluded, although broadly fallacious, contains fragmentary truth. It must have come within the range of the earlier pathologists—for no fact is more evident—that in tuberculosis and scrofula there is an unusual liability to enlargement of the lymphatic glands. Not knowing why in each division such enlargement occurred, and observing that in the coarser disease suppuration more frequently took place, medical scientists ascribed the differences to the varying intensity of unknown processes identical in kind. It is my belief, which I state with all due modesty, that by my experimental work I have wrested the knowledge of the true pathology of scrofula from Nature and placed it plainly and demonstrably before the world. Other and more detailed truths in connection with the disease undoubtedly remain to be discovered, and this task, almost impossible of accomplishment while the direction in which experimentalists should labour was unknown, has now become practicable.

The first point which engaged my attention in connection with scrofula was its possible septic origin. As in the previous article, I shall place the record of my experimental work in the form of question and answer :—

Question 1.—Is there to be found in scrofulous abscesses, or elsewhere in scrofulous patients, any form of life peculiar to the condition?—No. Forms peculiar to scrofula are unknown. The tissues may of course be septic, when bacteria will be found.

Question 2.—Have these any specific influence?—No. They possess merely that of common septic matters. Scrofula is not produced by their injection into the blood, nor if they be placed in lymphatic trunks or glands. Under these circumstances the results brought about are those we have seen to follow the injection of tubercular matter.

Question 3.—Is there any difference in the virulence of the septic poison, having relation to the parts whence it is taken?—Yes. Septic pus from suppurating lymphatic glands of a brute has a far more powerful action when injected into the lymphatic system of another animal than when taken from any other part.

Question 4.—What follows the injection of such pus?—The phenomena of tuberculosis previously described.

Question 5.—Is scrofula communicated by any such experiment?—No. None of the phenomena characteristic of scrofula follow. The conditions brought about are distinctly tuberculous.

Question 6.—Can scrofula be communicated by any other

means?—It cannot by simple transfusion of blood, although the animal receiving the scrofulous blood suffers for from twenty-four to seventy-two hours from general malaise. Transfusions performed once in three days on six consecutive occasions, will, if the quantity of blood used be sufficiently large, produce scrofulous symptoms. The quantity transfused should equal two-thirds of the whole normal amount in the animal injected. It is necessary to bleed the receiving animal until coma is produced before transfusing, and while the transfusion is in progress to permit the blood to continue running until the estimated quantity has been drawn. The scrofulous animal from which the blood is taken is, of course, sacrificed unless steps are taken while the transfusion is in progress to inject into its vascular system either blood or an artificial fluid. Scrofula can also be induced by exhausting processes, but not in the first generation, that is, in the first animal subjected to experiment. By giving a male guinea pig aphrodisiacs to ensure repeated copulation, and at the same time a lessened supply of food—about two-thirds its ordinary allowance, the following symptoms occur: alopecia, pityriasis, flaccidity of the sclerotic and cornea, photophobia, and occasionally varix. If nerve foods, such as the phosphates, phosphoric acid, &c., be employed, in combination with the application of electricity to the spinal column, these symptoms do not result. The influence of a vegetable dietary appears to be of paramount importance, for in strictly herbivorous animals, as those of the class mentioned, a far longer course of exhausting treatment is required to produce the phenomena specified than in the omnivorous rat. In the case of this latter animal I have found the usual number of days required to be eleven when kept on meat diet; in the former never less than twenty-one. Individual cases from each class of animal differ very widely, but the relative differences between the herbivora and omnivora is retained. When the rats were kept on a vegetable dietary their susceptibility to the exhausting processes was lessened. In some cases their power of resistance equalled that of the guinea pig. The explanation is probably to be found in the fact that unlike compounds more readily combine with each other than do like. The result of the experiments to the meat-gorging public is of significance.

Question 7.—When these phenomena have been brought about, is there any peculiarity in the blood?—Yes; there is a diminished number of red blood cells, and the majority of them appear undeveloped, with ill-defined form and lessened colour.

Question 8.—When the condition described as being produced by exhausting processes obtains in the male of a breeding couple, what will be the physique of their young, born after the incidence of such condition?—As a rule there is no structural disease detectable, either in life or *post mortem*, if the animals be killed. There is, however, a general physical deterioration rendering them liable to contract disease under slight influences.

Question 9.—What will be the effect on the young males of subjecting them to the same conditions as was the male parent? and what will be the effect on their own offspring?—The phenomena observable in the first generation will be intensified; the veins will tend to become varicose, the arteries to rupture, forming aneurysms, the capillaries to distend at any point of strain, as about joints. From this last, as well as from chronic inflammation, to which the capillaries are very liable, there frequently ensues hyperplasia, which in many instances becomes ossified. In the offspring, that is, in the third generation, I have frequently seen congenital disease of the skin, capillaries, and lymph glands. When not actually congenital such disease ensues from the most trifling irritant causes. The affections of the skin are chiefly alopecia, followed by pityriasis; of the capillaries, inflammation and ecchymosis; of the lymph glands, indurated enlargements, frequently becoming inflamed and suppurating.

Question 10.—Can these results be attained by means other than sexual exhaustion?—Not so readily. They can, however, by subjecting animals to operating circumstances, affording throughout the whole system lowered opportunities of maintaining vitality. Thus, if a male and female of cleanly species, as the wild rabbit, used individually and by their ancestry to breathe pure air and eat wholesome food, be taken from their warren, confined in a foul atmosphere, and fed on unwholesome food, not so poisonous in either case as to destroy life, we shall, after a few weeks, perceive a marked deterioration in their physique. The young of such a couple, subjected to exactly similar conditions as the parents, will very generally have their constitutions adapted to the change, and no marked disease will show itself. But let the circumstances be altered, still maintaining their unhealthy character, as by changing the diet, and the substitution of, say, the vicinity of a foul closet for the interior of a cellar as their place of keeping, and the disease-phenomena we have already noticed will be seen. By breeding under these changed conditions, and the rearing of offspring



thereunder, the latter are seen to be physically enfeebled, but not so greatly as the parents.

Actual scrofula is brought about more readily by a continuance of unhealthy conditions under a constant change of character, since the constitutions of the animals have not the time in which to accommodate themselves to these changes, than by unhealthy conditions of uniform character.

Question 11.—What is the condition of the tissues in scrofula, anatomically and chemically?—In the brief space at my disposal it is impossible for me to deal fully with this question. I purpose, therefore, giving the bare outlines of the more important among my rather voluminous notes, and dealing with them in a more detailed manner, in a subsequent article either here or elsewhere.

First, then, it must be understood that there are degrees of intensity in the scrofulous diathesis. When the condition is fully developed, the external characteristics in the brute resemble those found in the human subject. The joints are enlarged, especially those supporting the body. Attenuation is not general—on the contrary, there is a tendency to the deposition of adipose tissue.

The skin is always affected with pityriasis. In the brute this occurs over the whole body. In the human subject on the face and scalp chiefly. On section the epidermis exhibits a superficial irregularity—many of the cells are broken, others have an ill-defined outline, others again are placed almost edgways to the surface, so that such a section appears not unlike the side view of a cutting through geological strata that have been subjected to a natural upheaval.

Alopecia is very common both in induced and human scrofula. The hair is dry, the individual hairs are found to be brittle, and not infrequently split, or with clubbed tips; loss and irregularity of colour are very frequent but not general. Microscopically, the hair is seen to be roughened on the surface, and the canal is often occluded.

The claws of brutes under experiment, even when the scrofula is congenital, are seldom particularly affected. We shall see the reason of this presently. The nails in the human subject, on the other hand, are invariably involved in the general malnutrition, when scrofula is fully developed. The peculiarities consist in stunted growth, alone or combined with irregular laminæ. The nail presents one or more ridges having the appearance of the slates on the roof of a house. On section the malnutrition is evident, for the appear-

ance presented is that of so many interrupted strata, frequently three or four in the nail's length.

The sclerotic and cornea both in the brute and man are invariably flaccid. There is nothing, microscopically, peculiar in the eye tissues, save that the capillaries are always to some extent congested. This congestion is also found in the lacrymal gland. Irregularity in the position of the teeth is not by any means general, but malformation is frequently to be seen in hereditary cases. This consists of a ridged dentin and irregularity in the length of the enamel fibres. Early decay is pretty constant.

Allusion has been already made to the joints. The enlargement is lateral and due to pressure on the congenital cartilage. Microscopically, this cartilage in the brute shows a flattening of the columnar cells. In the human subject I have not had the opportunity of examining it. Enlargement of the finger joints is not hereditary in scrofula; it is produced by manual labour or exercise, and is best seen in the working classes. The bones of the arm in the human subject are never bent; in the case of the legs they are very frequently so. This is also, of course, due to pressure.

In the brute affected with congenital scrofula, the limbs are always more or less curved.

Microscopically, there are always to be seen interrupted laminae towards the external aspect of the bone. They commonly present the appearance of so many semi-circles and arcs united in a series of irregular figures, whilst here and there a nearly complete lamina is to be seen, having enclosed within it one or more of these figures.

The supporting framework of the fat is always attenuated, and the walls of the fat cells are weak, readily yielding under pressure. There is no diminution in quantity save after semi-starvation, but the fat formed is flabby and loose.

To the naked eye there is seldom anything unusual in the appearance of muscular tissue taken either from the scrofulous human or brute subject. But microscopically the case is very different. If the scrofulous diathesis be thoroughly developed, and the muscle from which the section is taken be not one that has been subjected to much healthy exercise, the fibres will be found thinner than they are normally, irregular in length, with ill-developed striæ in the striped muscles. These striæ are in some spots absent. The fibres themselves present here and there no lateral boundary line; while in scattered loci they are of such a diminished length as to resemble elongated cells.

In muscular tissue which has had its growth stimulated by exercise these appearances are absent.

With regard to the structure of the blood-vessels, I have found the muscular fibres affected in the manner mentioned when speaking of muscle. Neither the walls as a whole, nor their individual coats, were throughout the system thickened.

Here and there, in the course of a muscularly-walled vessel, the walls were thinner and the calibre of this part of the vessel enlarged.

Yet, such increased thinness never being seen without the corresponding enlargement in calibre, it would be an unjustifiable assumption to conclude that the latter is the consequence of the former. Rather would the thinness appear to be due to the distension, the real cause of the yielding being cellular modification, and not alteration in bulk.

The calibre of the skin capillaries in scrofulous brutes and human beings frequently deviates from the normal. The general tendency is in the direction of narrowing; but in places they are distended, while here and there broken down completely. The blood-channel forms a sacculated enlargement, continuous with one or more vessels.

The blood circulation through the capillaries is very generally slow.

The capillaries of internal organs and parts I have not found markedly altered in character.

I have never succeeded in obtaining fresh and unstained sections of the brain and spinal cord from the scrofulous human subject. Such sections alone can give an accurate idea of histological structure. In the brute but little deterioration is apparent. The cells are frequently ill-developed, with, in many instances, fewer branching processes. This is the only change I have up to the time of writing been able to detect.

Cultivation of the brain's cellular structure is, of course, quite outside the range of the laboratory experimentalist's range of research. Thus much, however, I have found to be true—scrofulous children, stupid and dull, increase in intelligence under proper mental stimulation. This intelligence may be of a very high order, and is then invariably associated with what we are justified in considering as external indications of good brain-size and formation.

The stupidity in such cases we must ascribe to cellular faults, not errors in the bulk or design of the brain.

These observations agree with many biographical facts as to the

dulness of persons in early life, who afterwards attained great eminence on account of their high mental powers.

It may be said, in conclusion, that scrofula, as induced in the laboratory and as seen in the human subject, present, with the exceptions mentioned, identical pathological appearances.

In considering the morbid anatomy of scrofula, it must be remembered that the tissues are influenced directly by surrounding agents.

Thus, while muscular deterioration is well marked in scrofulous persons leading an inactive life, so far as any particular muscle or set of muscles is concerned, should it be exercised it will follow the common law of physiological growth, and the produced tissue will be healthy in character.

Again, it is, as before remarked, chiefly amongst the working-classes that we find enlarged finger joints. The same remark applies, though not with equal force, to cases of enlarged joints at the knee and ankle, and to bending of the leg bones. Very many working-people look well after their offspring, and, finding the legs yield, do not permit the child to walk to any extent. The skin and hair again are altogether modified by the treatment they frequently receive from those who study their personal appearance. And the same may be said as to the preservation of the teeth.

It would appear to be in obedience to the common law of stimulation and growth through use that the claws of brutes are less affected by scrofula than the nails of the human being.

The chemical deviations from the normal occurring in the bodily tissues of the scrofulous brute may be summed up as a lessened percentage of mineral constituents and animal compounds, and an increase in the percentage of water. There is no compound or substance added, and there is nothing absent which is found in the normal state. For details on this very important subject I must refer the reader to a future article.

Question 12.—Are scrofulous animals especially liable to the formation of tubercle?—Yes; the formation of tubercle readily follows any slight irritant conditions, and it speedily subsides when the conditions are withdrawn. It has been in all my observed cases caused proximately by lympho-vascular inflammation.

Scrofula is very readily developed in animals in whom the hereditary tuberculous diathesis obtains, provided there be no extensive deposition of tubercle. It can be brought about by the means already described, but every care has to be taken to avoid lympho-vascular inflammation. This is necessary, because the lymphatic

system being already affected, any further weakening of the body is felt primarily by this system, and the tendency in all such cases is to the direct establishment of acute tuberculosis. When this last disease exists it is next to impossible to establish scrofula, unless the tubercular affection be first subdued. The means adopted for such establishment of scrofula increase the intensity of the tubercular disease, and death results long before that general bodily innutrition peculiar to scrofula can be brought about.

Question 13.—Is there lessened absorption in scrofula?—There is not any lessened absorption of non-fatty articles of diet. A scrofulous animal will consume non-fatty food, with a minimum of waste in the fæces; but fatty food is absorbed in small amount only.

Question 14.—To what is this due?—It would appear to be owing to insufficient secretion of the pancreatic fluid and of bile. By establishing pancreatic and biliary fistulæ in cats, as mentioned under tuberculosis, I have found these secretions reduced, as in the disease last named.

Question 15.—Are the intestinal secretions and contents acid in scrofula?—The secretions are not; neither in the herbivora have I found the contents so until the middle of the small intestine. In the omnivora the contents are distinctly acid. This acidity is, it would appear, due to the splitting up of the fatty portions of the food. It is increased by a fatty diet, and reduced to a minimum by one of a vegetable nature.

Question 16.—Must we, because of the preceding facts, conclude that scrofula includes tuberculosis?—No; true tuberculosis may be superadded to scrofula, but this is altogether exceptional.

Question 17.—What, then, are the pathological differences between tuberculosis and scrofula?—Scrofula is a disease of nutrition; the tissues are ill-formed, and tend to break down under the slightest strain, but there is no single system affected, as a rule, more than the rest. The formation of tubercle is due to weakness of the lympho-vascular systems, rendering them liable to give rise to inflammation under comparatively trivial exciting causes. This general malnutrition does sometimes affect a particular system or organ, and the condition then is perpetuated in the individual, presumably by the law of cellular assimilation. When the lymphatic system is especially affected, true tuberculosis is the result.

Tuberculosis is not primarily a disease of general nutrition but of the absorbent or lymphatic system. It results in tubercle formation and impoverished blood, hence tending through inheritance

to malnutrition and scrofula; while scrofula through malnutrition tends to produce a weakened absorbent system, or tuberculosis.

Hence it is in old inherited cases, whether induced in the laboratory or seen at the bedside, that tuberculosis and scrofula are most generally seen in the same being. Each tends to produce the other condition. Scrofula may be considered as survival amid general unhealthy surroundings—as the result of processes which, had they been acted on by the ordinary causes of fatal disease, would have terminated the subject's life.

There can be no doubt that if the ordinary causes of death were less frequent in operation scrofula would increase. Its incipient subjects are mowed down by tens of thousands in our slums and among the very poor in country districts. Scrofula is a modified physiological state; it is the result of the opposed action of agents tending to health and enfeeblement respectively; it is essentially a compromise between, and therefore a union of, these two. Its principle is best seen in the example of its production amongst peasantry breathing pure air, drinking pure water, and taking plenty of exercise, but miserably fed for the work they have to perform and the exposure they have to undergo.

Question 18.—What are the common causes of scrofula in the human subject?—It would appear to be the consequence of hereditary progressive atony. I have never seen scrofula result in the healthy child of healthy parents from its being subjected to unhealthy conditions of life. It is possible that it may thus be produced in the first generation, but such cases have not come within my range of research. States of life, all of which lead in one or more directions to enfeeblement, while in others they are healthy, and changing from time to time their particular enfeebling character, appear to be most favourable for the establishment of the disease.

The change mentioned is essential, for a continuance of unhealthy conditions identical in kind results always in one of two ways—(1) death, that is extermination, or (2) physiological accommodation.

Cases exhibiting scrofula-producing conditions might be adduced by the score. I will give here a few in whose accuracy I have every reason to believe:—

CASE I.—R. T., aged twenty-four, costermonger. Scrofulous appearance. Born in Lambeth, and brought up in one of its slums; whole family of seven slept in one room. Has been from infancy subject to inflammation of lymphatic glands; in ninth year two glands in neck,

and in eleventh year a gland in left axilla inflamed and suppurated. Parents alive; were immigrants from Ireland. Mother healthy. Father phthisical, skin affected with pityriasis simplex; was born in Cork; his father had been a merchant seaman, and had suffered from scurvy at sea three times.

CASE II.—S. L., cabman, aged thirty-six. Very heavy and dull countenance. Scrofulous history from birth; abscesses in various parts of his body at different times; ankle joints swell after the least exertion in walking. Born in Staffordshire; parents peasants in that county; poorly fed, and often ill from want and exposure. Paternal grandfather had been a coal miner; suffered from phthisis; had given up mining and settled on farm work; after this, disease made no progress and he married, his wife being the grandmother of the patient.

CASE III.—G. T., ostler, aged forty-one. Full scrofulous diathesis. Born "somewhere in Islington." Father had been a public-house loafer, minding cabmen's horses, &c.; nearly lived on beer; had no recollection of his mother. Paternal grandfather had been a sweep; died of asthma.

CASE IV.—N. A., gentleman, aged thirty-eight. Stunted and flabby appearance, but scrofula not well marked in conformation of features. Mother came of very healthy family. Father a beneficed clergyman, "delicate," hard student; health suffered in consequence, and had to give up work and retire to Continent, where N. A. was born. Paternal grandfather a gentleman farmer in Devon; very great eater; hard drinker; inclined to other excesses; has been described to his grandson as bloated and gouty.

Question 19.—Can scrofula be developed in utero as a result of temporary ill-health or nervous shock on the part of the mother, the progenitors being constitutionally healthy, and the surroundings conducive to bodily well-being?—Whether true scrofula may result from mere temporary innutrition of the healthy mother, and consequent innutrition of the foetus, is an unsettled question. I cannot regard the evidence on this point which I have been able to collect as in any way conclusive. Still more unsatisfactory is that concerning the influence temporary innutrition of the father may have in the production of this state. The balance is certainly at the present moment in favour of the negative view, but more extended research in this particular direction may possibly alter the position of the scales. That such temporary innutrition will produce tuberculosis I have proved to my complete satisfaction, and this is what might be expected. Tuberculosis is the result of direct atony, and, as a consequence, any enfeeblement must very

readily affect the lymphatic system; but scrofula is the result of enfeebling processes acting on the one hand, and nutritive processes on the other, the enfeebling being predominant and resulting in a physiological modification by which life can be maintained on a state of lowered nutrition of the whole body.

I have seen cases which leave no doubt in my mind that scrofula can result from nervous shock to the mother. There would appear to have been an arrested nutrition. All the children I have seen affected through this means have been stunted and ill-developed.

Nervous anxiety on the part of the mother will frequently produce tuberculosis in the child, but never scrofula. Hence the large number of tuberculous cases seen amongst the illegitimate children of respectably brought-up girls.

In seeking for an explanation we must remember that brain-worry is blood exhaustion. That the maternal nervous system can, through its direct connexion with the nervous system of the child, control its nutrition is proved by the beneficial effects to the child in utero, resulting not merely from nerve-foods, when used by the mother, but also from the application of electricity; secondly, from the inheritance of psychoses, due to faulty nerve or brain nutrition; and lastly, from the effect on the child's body of strong maternal mental impressions, which cases, although they may excite the smile of flippant biological babes and sucklings, are too numerous and credibly witnessed to admit of the scientist's doubt.

Such an exhausted nervous influence, then, may in the cases of tuberculosis produced by nervous anxiety play an important part.

Question 20.—Is scrofula ever seen without some affection of the lymphatics?—The scrofulous condition, which is innutrition, of necessity includes innutrition of the lymphatic system, and consequent lymphatic weakness. The tendency to tuberculosis is therefore marked in all scrofulous subjects; but if exciting conditions be absent, actual formation of tubercle may never occur, while the diseases especially characteristic of scrofula may obtain in force.

The pathological relations of the scrofulous, tuberculous, and other conditions of malnutrition, absorption, and secretion, to each other, will now be generally considered.

Scrofula is the term applied to the condition of the body when the powers of general nutrition are deficient.

In tuberculosis absorption only is affected; in scrofula the absorbed material is not elaborated. The red blood-cells are immature, and



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the spleen is comparatively inactive. Scrofulous patients are not, as a rule, thin; the tissues, however, lack tone. Scrofula may be brought about by conditions lowering the nutritive qualities of the blood and the chemical constitution of the tissues.

A tubercular animal may become scrofulous. The conditions appear to be—such mode as does not afford opportunity for the development of tuberculosis, joined with a plenteous dietary, and an abnormal drain on the blood.

The proximate cause of scrofula is deficient nutrition. This may be expressed as a lowered vital tone of the whole of the tissues. The starting point may be—(1) exhaustion of nerve force through elimination of elaborated nerve compounds and a consequent inability on the part of the nervous system to maintain its trophic influence; or (2) exhaustion of blood materials, by which the tissues are deprived of their proper food; or (3) blood starvation, resulting in the same manner as its exhaustion; or (4) the presence in the blood of improper, that is, poisonous materials.

The process of production of scrofula is that concerned in the causation of all disease, save accidental, zymotic, and congenital malformations; and even these three may result in the establishment of the same process. In order to fully grasp its nature, we must bear in mind the relations to each other borne by the body's various organs. The living animal body is a mechanism, and as such we must view it. The disease process may commence at any part of this mechanism. Selecting as the organ the most exposed to injury—the stomach—let this be injured, say, by alcohol, and digestion will become impaired. Impaired digestion means lessened nutrition of the blood. This last must find a result in poorer tissues. These tissues include all those entering into the structure of special organs. Then the nervous system will be weakened and its trophic influence lessened. The kidneys will be weakened, and poisonous material remain in the blood. So with the liver, while the lessened secretion of bile will still further impair digestion. So with the starting organ—the stomach. The blood now poisoned, offers not merely less nutritious, but poisonous food to all the tissues. And thus round and round in an ever-swifter circle the process sweeps on. But while this is continuing, extrinsic causes may be at work, and being brought to bear on a particular organ, may produce more intense disease, at the first purely localised. Thus exposure to cold may engorge the lungs—pneumonia. If a large extent of lung be affected and the tissues be

feeble, death will generally result from the disease named. If the engorgement be more limited, and the tissues weak, secondary processes ensue, the result being phthisis. But should the engorgement be limited, and the tissues in a fairly tonic condition, resolution will ensue. So congestion of the kidneys following exposure to cold will terminate either in resolution or Bright's disease, according to the extent of the engorgement and condition of the parts.

By experiments on living animals I have found that when a thoroughly unhealthy state of the blood and tissues had been produced in any two, male and female, the young of these invariably exhibited a greater readiness to succumb to any noxious influences brought to bear on them. Before experimenting on the offspring I never failed to detect in them impaired general vitality. In some cases there was found great localised debility, either in the kidneys, stomach, heart, or other important organs. And this local weakness readily caused its seat to be more markedly affected by debilitating treatment than other and more healthy organs.

The conditions necessary for the development of such general ill health have been already mentioned. The detection of the latter is not always an easy task where no organic disease exists; but where any doubt is felt, a just decision can be readily arrived at by killing the animal and examining the tissues of the body with the microscope. An autopsy at which all observations are made by the naked eye must by its very superficiality, and consequent unreliability, prove a failure. The microscope will very frequently reveal to the observer abnormal states of the cells, which *en masse* present nothing peculiar to the eye, and individually are undistinguishable by it. Local disease when produced I have not found to be inherited by the offspring of the animal under experiment; but I have invariably seen that the parts in the offspring corresponding to those affected in the parent, become more readily diseased than in the case of animals from parents not so affected. But, and this is of the utmost importance, if the offspring in question were particularly guarded against disorders of the parts concerned, they escaped; but their own families, on exposure to producing conditions, readily succumbed.

It is, then, to absence of such exciting causes in the human family that we must attribute the escape of children from the organic diseases affecting their parents, and to the recurrence of such causes that the descendants of the third and fourth generation suffer as did their progenitors.

The doctrine of the survival of the fittest is one demonstrable by experience. Such survival is due to gradual modification of the animal's constitution to agree with its surroundings.

But survival may not be identical with healthy vitality, and under certain conditions the resulting beings may be of an altogether low development. Further, it is the unfit who yield most readily to disease; and it is in these and their posterity that disease prevails. Even here, while the whole system of the descendant may be morbid—immensely more so than in the ancestor—it may not so readily become subject to acute affections under similar conditions as its ancestor. Thus, I have found, apart from experimental research, that children and grandchildren of country persons, brought up in London slums, prove far more susceptible to fatal disease than those whose predecessors have for generations lived in those dens of filth. But in the latter there was an altogether lowered physique.

*The Production and Relationship of Tuberculosis and Scrofula.*

[= signifies produces.]

1. *Tuberculosis* has for cause—Weakness of lymphatics =  $\left\{ \begin{array}{l} \text{Non-absorption in intestines.} \\ \text{Non-absorption of effete cellular matter.} \\ \text{Lympho-vascular inflammation.} \end{array} \right.$

Non-absorption in intestines = impoverishment of blood = Malnutrition.

“ of effete cellular matters = Miliary tubercle.

Lympho-vascular inflammation = Caseous tubercle.

Malnutrition.	Stomach, &c.	= Impaired digestion	= Lessened absorption.
	†Kidneys,	= Poisoned blood.	
	Liver, Pancreas, &c.	= Impaired digestion	= Lessened absorption.
	*Lymphatics	= $\left\{ \begin{array}{l} \text{Lessened absorption.} \\ \text{Lymphatic inflammation.} \end{array} \right.$	
	Nervous system	= Lessened trophic influence	= $\left\{ \begin{array}{l} \text{Lessened nutrition.} \\ \text{Absorption and excretion.} \end{array} \right.$
	General tissues	= General atony.	

\* Lessened absorption in intestine = Impoverished blood = Malnutrition.

\* Lessened absorption in tissues = Miliary tubercle.

\* Lymphatic inflammation = Caseous tubercle.

† Poisoned blood = Malnutrition.

*Completion of the Circle,*

With special direction to disease of any particular organ, through exciting conditions.

2. *Scrofula* as above, commencing at Malnutrition.

The doctrine of the survival of the fittest can be but rarely exemplified in the human family, in a natural manner, under the

conditions of civilisation; for, by the continual inpouring of new blood, through marriage, every human being possesses in himself the constitutional tendencies of many divergent races of ancestors, and these conditions prevent both extermination and the thorough modification of the system. But the partial modification is to be found on every hand, and notably in those scrofulous patients who, save for the phenomena peculiar to their condition, fulfil all ordinary life-functions in a condition of health.

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#### URTICARIA OCCURRING IN INFANCY.

IN a communication read before the Clinical Society of Paris (*La France Médicale*, Août 25, 1885), Dr. J. Comby states that Prof. Bouchard, in his researches upon dilatation of the stomach, has observed that a certain number of these patients are subject to more or less annoying outbreaks of urticaria. It is not at all surprising that a disease which has for its principal and immediate consequence faulty elaboration of the ingesta should awaken cutaneous manifestations like urticaria, when we see temporary digestive disorder (*embarras gastrique* and indigestion) accompanied by the same eruptions. This urticaria, due to lesions or functional disorders of the stomach (like those which supervene after tapping hydatid cysts), is very probably a toxic urticaria, due to the fact that the skin serves as a route of elimination to the poison elaborated in the digestive passages, and taken up into the circulation. This, at least, is the rational explanation which M. Bouchard gave to this phenomenon in a paper recently read before the Faculty of Medicine. Dr. Comby tried, in a certain measure, to apply to the infants coming under his observation the results which were obtained from adults by Prof. Bouchard. The dilatation of the stomach, encountered so often among the latter, Dr. Comby had found with almost equal frequency among infants of the poorer classes, subjected from birth to defective alimentation. Among animals, also, he had found, as a consequence of dilatation of the stomach, multiple disorders in many of the organs and tissues. The diverse eruptions grouped by authors under the name of "*gourmes*" (porrigo, scald head, &c.) appear with extreme frequency in little patients with dilatation of the stomach; but, of all these eruptions, that which had struck him most by its objective and subjective characters is urticaria, of which he reports five cases.—*Phil. Med. Times*, October 3, 1885.

## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

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#### RECENT WORKS ON GOUT.

1. *Gout in its Clinical Aspects: an Outline of the Disease and its Treatment, for Practitioners.* By J. MORTIMER GRANVILLE, M.D., M.R.C.P.L., Mem. Corr. Étranger Société Clinique de Paris. London: J. & A. Churchill. 1885. Pp. 307.
2. *A Treatise on Gout, Rheumatism, and the Allied Affections.* By PETER HOOD, M.D. Third Edition (Revised and Enlarged), with a Chapter on Sleep. London: J. & A. Churchill. 1885. Pp. 419.
3. *Gout, and its Relations to Diseases of the Liver and Kidneys.* By ROBSON ROOSE, M.D., F.R.C.P.E. First and Second Editions. London: H. K. Lewis. 1885. Pp. 160.

THE publication of three English works on gout in one year is in itself a sign of the increasing prevalence of the disease of which England may almost claim a monopoly. The authors, too, are convinced that gout "has increased, is increasing, and ought to be diminished;" though they do not altogether agree as to the best means of effecting the desired diminution. This fact alone makes the subject important and interesting; but, for another reason, gout has a special interest for us. It is one of the few remaining diseases which have hitherto escaped the ravages of the bacillus. No pernicious microphyte has yet been discovered to which the pangs of gout can be attributed; and pathologists are obliged to content themselves, in this instance, with a mere mineral *materies morbi*. Whether the treatment of the disease would be advanced by a demonstration that uric acid is alive is, if we may judge from experience in other cases, at least doubtful; but at present there is no need to discuss this question. We shall proceed to examine what our three (occasionally conflicting) oracles have to tell us.

1. Dr. Granville's book shows a slight tendency towards the sin that doth so easily beset the specialist. Now and then he appears

as if he looked upon mankind through uric acid lenses, as your thorough-going gynæcologist is so apt to survey all the ailments of his fair clients through a speculum. There is a tendency, too, to heteromorphic growth in his book; as where seventeen pages of an essay on Sleep are intruded into the eighth chapter, and a "synoptical index of drugs" occupies twenty-six pages. But it is a good, practical work, well arranged, easy to read, full of experience, and containing a summary of most of what is known or thought about gout. The first part—"Facts and Indications"—occupies 170 pages; the second—"Treatment and Formulæ"—about a hundred. The latter includes an excellent chapter on baths and mineral waters.

Gout is an organic disease; that is, it is "dependent upon some organic peculiarity which favours excess of production, or defect of conversion, or re-absorption of uric acid." This acid exuded into the tissues, either free or in combination with sodium, irritates as a foreign body, and produces the local manifestations of the disease. So far all are agreed. Dr. Granville maintains, in addition, that the injury done by deposited uric acid (or urate) is mechanical merely; that, except mechanically, it does not interfere with the performance of function. Its presence in the blood is dangerous, not intrinsically, but from its tendency to crystallise and to be deposited somewhere. But this presence is the "essential morbid condition" in gout, and due either to excessive formation, or defective destruction, or re-absorption in the kidney. Whether the perversion of function which leads to the presence of the acid in the blood is an error of nutrition (in liver or spleen) or of excretion (in kidney), is a question which has not yet been satisfactorily answered. In Dr. Granville's opinion it is the liver which first fails in gout:—

"Either it has too much to do, or its power is diminished, so that uric acid accumulates in this organ, and is not eliminated from the blood. If it can be got rid of, either by the kidneys or by a catarrh of the respiratory organs, no great harm ensues, and the liver may recover itself; but if uric acid be not thus discharged, urates will be thrown out into the interspaces of the tissues somewhere, and an attack of the malady with localised symptoms will occur. The nature of the attack will be determined by the condition out of which it arises. If there has been a sudden accumulation, or recent re-absorption, of uric acid, the illness is likely to be acute like an eruptive fever. If the accumulation has been of slower formation, the paroxysm may be sub-acute, or there may be no

greater disturbance than is contingent upon the *goutte à goutte* of new material at the seat of old deposits, thus constituting the phenomena of chronic gout. . . . I do not think the strong leaning of contemporary theorists to a hypothesis of purely or mainly nervous causation is warranted by the facts. The study of the disease from a clinical standpoint tends rather to prove the general truthfulness of the older conjectures about gout than to support more recent theories" (p. 14).

It follows that the indication for treatment during the attack of gout is to promote the discharge of the uric acid from the system. This may best be effected through the kidneys. It is true that in certain cases some does make its way out through the skin. "Nevertheless, it is hopeless, and would be dangerous, to try to relieve the patient by sudorifics; nor will drastic purgation answer the purpose." Diaphoresis in gout is rather inimical than favourable to cure, because it reduces the urinary flow; and it is doubtful whether the intestinal glands are capable of excreting uric acid to any considerable extent. Some physicians, indeed, hold that uric acid may come away with the bile, and try to evacuate it thus; but, "beyond question, the proper way of exit for uric acid and the urates is the kidney." Ordinary diuretics, however, are inapplicable, because they are irritants. The indication is to "flush" the kidneys, not to stimulate them. For this purpose warm dilutions are appropriate and the best drugs are ammonium chloride and potassium chlorate. At the same time the decomposition of the sodium urate in the blood may be attempted; and to this end, Dr. Granville considers iodine best adapted, exhibiting it with the salts and glycerine.\*

Should this treatment fail, Dr. Granville "falls back" on colchicum. He does not object so strongly to the use of this drug as Dr. Hood; but he evidently does not like it, and would rather do without it. Colchicum, he says, depresses the heart, often seriously, and often irritates the bowels; and tends to congest the kidneys, probably without promoting the discharge of uric acid or urates. It subdues fever and relieves pain, "but the good effects of the drug are quickly lost, and it cannot afterwards be safely pushed far enough to recover its influence." That it should hold its ground

\* R. Ammonii chlor. ʒiv.

Potassii chloratis, ʒij.

Tinct. iodi, ꝑ cxx.

Glycerini, ʒi ss.

Aquæ, ad ʒxvi—Misce.

F. Mistura, cujus sumantur cochlearia duo magna quartâ quâque horâ ex aquâ.

in the treatment of gout, to the extent it does, proves its potency, which it probably owes to its specific influence upon the liver. It has had many alternations of favour and disfavour in the opinions of physicians; and at present authority seems to be against its use. For other methods of treating the acute attacks of gout, alternative but inferior to the exhibition of iodine, we must refer our readers to the work itself (pp. 46 *et seq.*).

Between the attacks the wise physician finds his opportunity for remedial treatment; not by limiting the supply of nitrogenous or other nutriment, and thereby impairing the resisting power of the constitution, but by endeavouring to rectify the faulty action of the liver, on the assumption that gout primarily depends upon hepatic misconduct. We are to avoid the free employment of so-called "cholagogues," remembering that the liver is not merely, or even chiefly, an excretory organ. The digestion of fats, for instance, cannot be carried on without the free secretion of bile; and the biliary salts, especially the "taurocholate of soda and the phosphates," are not excremental, but nutritive. In fact, cholesterine, "the ultimate product of the disassimilation of nervous tissue," is *the* excrementitious product of hepatic action. The author is of opinion that the accumulation of uric acid in the blood is due, at any rate, in part to deficiency of biliary salts; and, on this opinion, he founds his treatment of the "intervals" in gout. He administers taurocholate and glycocholate of soda, prepared from ox-bile or pig-bile, in pill, with food, twice or thrice daily, over long periods, until the urine shows permanent improvement of the condition. As to dietetic management, Dr. Granville is altogether opposed to "regulation diets." He advises as rich and varied a diet as the appetite demands or the palate desires, with a moderate quantity of stimulants taken with food.

"It is a mistake to suppose that benefit results from what is called 'moderation' in the pleasures of the table, but which is really a low diet, as generally recommended. I do not think physicians can be in full possession of the facts with regard to the lives of the persons who submit to this *régime*. They are anything but happy, and I am sure they are not either healthy or strong. They wander about from doctor to doctor, or try the nostrums recommended by their friends, or they settle down to a life of weakness and suffering, and, not uncommonly, succumb to maladies which are of very small moment in themselves, but attacking a half-starved and atonic organism, become formidable. A great deal is said and written against the evils of over-stimulation. I believe the



evils and drawbacks of a life devoid of energy and, as it were, pitched at a low tone, with less than the normal amount of tension, are greater" (p. 61).

In his chapter on "chronic gout," Dr. Granville instances "Charcot's disease" as an example of a "new" disease, which is really a combination of chronic gout with some other affection. This special joint-affection he regards as the product of a graft of syphilitic taint upon a stock of hereditary gout. He contends that when the group of symptoms (which we need not detail here) presenting the clinical features of Charcot's disease occur, there is in all or most cases, a taint of hereditary gout. A condition closely resembling that described by Charcot is not rare in cases of chronic gout, "particularly when accompanied by nerve disturbances." A combination of gout, syphilis, and nervous derangement constitutes Charcot's disease.

A short chapter (XII.) on the connection between lead-poisoning and gout points out that a combination of the two intoxications is possible, and specially likely to occur when a person is exposed to the causes of both; and the existence of one of these affections increases the liability to the other. It is in this combination of diseases that nervous complications, as insanity and chorea, are most to be expected. Of one connection between gout and syphilis we have already spoken; and shall only add, that "when a gouty patient contracts syphilis, that disease usually assumes a form which is characterised by a very wide distribution of its effects over the organism, and by the exhibition of a peculiar affinity for the nervous centres; and, if I may venture to generalise, particularly the centres of co-ordinative and sensori-motor activity, commencing on one side, nearly always that on which the original chancre existed" (p. 162).

In the second chapter of the second part, under the head of "treatment between attacks," Dr. Granville returns to his objection to "dietary schemes." He points out, as to restrictions on the use of sugar, that the patient must eat starch, which is immediately converted into glucose; so that we cannot exclude sugar if we would. It seems to be demonstrated that sugar does not increase the production of uric acid. Böcker maintains that it actually reduces it. Dr. Granville has recommended the free use of sugar as a stimulant of the kidneys in gouty cases, and, as he believes, with advantage. Again, to restrictions upon the liberal use of meat he opposes the fact that the carnivora produce uric

acid in infinitesimal amounts, but urea in abundance. Flesh food, therefore, he argues, is not injurious in gout. Excess of nitrogenous food may result in accumulation of urea in the system; "but the idea that it can produce uric acid . . . in gout falls to the ground with the hypothesis that uric acid is imperfectly oxidised urea, or a disassimilitative material in process of metamorphosis into urea—a hypothesis which must certainly be set aside as having no solid foundation in fact" (p. 199). We cannot trust ourselves to summarise Dr. Granville's further heretical teaching as to the use of stimulants; we must let him state it in his own words:—

"Again, that stimulants are not injurious in virtue of their containing alcohol is proved by the immunity from gout enjoyed by spirit-drinking peoples; as those of Scotland, Poland, and other countries. That the *imperfectly fermented* wines are injurious is evident; but the bad effects they produce are not due to the alcohol or the sugar they contain, for neither of these elements, as we have seen, is injurious in itself, but to the process of fermentation set up in the stomach by the introduction of a material which is, as it were, charged with the potentiality of a destructive metabolism. I have for many years past held this view, and I have been much gratified to observe, in the lectures and papers on gout published somewhat recently, evidence that what I believe to be the facts about food and stimulants are beginning to be recognised. Even port wine, strange as it may seem to say so, is not merely innocuous, but decidedly beneficial in gout, if it has been kept long enough in the wood before bottling to ensure the completeness of the process of fermentation. It is either the state of fermentation or the ferment that does the harm" (p. 199).

We conclude our notice of Dr. Granville's valuable work by joining in his protest (in his final chapter on "Baths and Waters") against the growing practice of recommending patients, gouty or otherwise afflicted, to seek at foreign spas what they could as readily and as effectually obtain in the United Kingdom. We, too, ask—"When will the profession and the public begin to perceive the folly of going abroad in search of what can be found in as great, if not greater, perfection at home?" and, in our question, we mean Ireland by "at home." "It is simply nonsense to think and speak as though anything in the way of mineral waters for bathing and drinking purposes which can be found abroad may not also be found at home." And again, "except in a meagre, half-hearted way, we do not take advantage of the opportunities at our command to

render the watering-places in our midst centres of fashion and entertainment."

2. Dr. Hood's Treatise (in its third edition) belongs to a class of medical works which is, we regret to observe, becoming rarer year by year. The results of long and carefully garnered experience are communicated to a grave and desperately earnest profession in an easy, pleasant, almost gossipy style, delightful to read and profitable to study, enlivened with anecdote and quotation; in which, while the "*dulce*" abounds, there is enough of the "*utile*" to refute Ovid's libel which the author himself quotes:—

"Tollere nodosam nescit medicina podagram."

To the original treatise on "Gout and Rheumatism" a chapter on Longevity was added in the second edition and remains in the third; and another chapter, on Sleep, appears now for the first time.

That gout is increasing in prevalence and fatality in England and Wales is clear from a table which Dr. Hood has compiled, showing the number of registered deaths due to this cause from 1859 to 1868, and the ratio of these deaths to corrected population. The increase is marked, the deaths rising from 238 (12 per million) in 1859 to 393 (18 per million) in 1868. Another table shows deaths for twenty-one years (1848 to 1868) according to sex and age; the total being 5,623; 4,583 males and 1,040 females. From this table it appears that "gout kills between four and five males to every female, and that the tendency to death is the most conspicuous between the ages of 55 and 75." It shows, too, that women make a better fight against gout than men, fewer dying at the lower ages. If this difference is due, or so far as it is due, to the wholesomer habits of life of females, it would seem to follow "that gout is more dependent upon habits than upon inheritance; or, at all events, that habits must claim at least an equal share with inheritance in its production and development." At this early stage our author strikes his first blow at his great enemy, *colchicum*. The mortality from gout is considerable; but it would be much less but for *colchicum*:—"So far as my own experience extends, I have never seen a case of gout end fatally, however severe the paroxysms, so long as *colchicum* had not been administered for their relief. I have known many persons who have sunk under the malady; but in every case they have been in the habit of relying upon *colchicum* as a means of cure, and in many instances they have been their own physicians."

Dr. Hood does not believe in "change of type." Men and their maladies are very much the same now as they always were. It is the doctors that have changed, and not in all things for the better. The abandonment of blood-letting, for example, has not been a therapeutical improvement, however the abuses of the practice may have demanded correction. Dr. Bright was of opinion that cardiac affections had become more fatal since the lancet had been disused; to these Dr. Hood adds "cerebral affections, terminating in mania or paralysis," and diseases of the kidneys; and the mischief has been aggravated by the coincident disuse of honest purgation. He discusses this subject at some length (and, may we suggest, a little irrelevantly), in connection with heart-disease in gouty patients. Heart-symptoms in gout are not forms of gout, but are results of gradual congestion of other viscera, "especially of the liver, and of the kidneys consecutively to the liver;" and they are to be relieved, not by specific treatment, but by removing the congestions which produce them.

The skin affections of the gouty, on the other hand, are forms of gout, and demand the treatment proper to gout. If relieved by local applications and arsenic, or other non-specific drug, they are apt to leave behind evils greater than themselves. Even malignant disease has, more than once in Dr. Hood's experience, followed closely upon the repression of gouty eczema. It follows that the treatment of this most distressing group of affections incidental to gout demands the utmost attention to the general health, and "the functions of the great emunctories." In many of these cases an issue or seton may be found beneficial.

The indication in the treatment of gout is to remove the *materies morbi* with as little damage to the patient as possible. Relief of suffering will follow judicious efforts in this direction; but to seek merely relief from pain, without attacking the cause of pain, is worse than futile. "Colchicum," says Dr. Hood, "is a specific for this purpose—it will relieve the pain of gout easily and quickly; but if we resort to it without first endeavouring to lessen the plethora existing in the capillary vessels which are unduly distended, and which, by their mechanical pressure on the subjacent nerves, are the immediate causes of the suffering," we shall do more harm than good. In his experience "gout can be as quickly and more surely cured without the agency of this drug than with it." "Its ultimate tendency is to destroy the integrity and vitality of the blood," and to render "the attacks of gout more frequent,

but subsequently more difficult of removal." In Chapter VII. will be found a brief statement of the opinions for and against the use of colchicum, from its re-introduction into medicine by Baron Stork, in 1763, to the present time. We need scarcely say that Dr. Hood sums up against the accused strongly and decisively. He himself recommends calomel instead. His chapters on treatment well repay perusal. We shall content ourselves by mentioning two other recommendations—the application of *whisky* to the painful joints; and, in chronic cases, of *sulphur*.

In the chapter on prevention we are warned as carefully by Dr. Hood as we had been by Dr. Granville against over-abstemiousness. Long intervals between meals should be avoided. French cooking is preferable to English for gouty patients:—

"The French have taste in all they do,  
While we must go without.  
Nature to them has given *gout* !  
To us she's given *gout* !"

"To feed coarsely on underdone lumps of beef and mutton, and to call this 'plain, wholesome living,' is an outrage upon common sense of the grossest description." It is noteworthy that gout in vegetarians, individual or national, is almost unknown; but Dr. Hood spoils the effect of this useful hint by an unkind inference from "the acts and writings of vegetarians," that a vegetable diet is not so capable of sustaining mental as bodily vigour. To abstinence from *sugar* our author is strongly opposed; and he states that he has noticed inferiority in the texture and appearance of the skin in those members of families who ate no sugar. Sugar has no ill effect in gout. He is at one with Dr. Granville also as to the use of alcoholic drinks; holding, like him, that fully fermented wines—port included—are innocuous.

From the chapter on "Complications" we extract a passage illustrative of Dr. Hood's style and, perhaps, of his willingness to "pick out treasure from an earthen pot." We fear the remedy for rectal hæmorrhage, suggested and even recommended, is like Friar John's charm against bullets in battle, useless to sceptics:—

"One very remarkable case I will relate; and I would not venture to do so if I could not entirely rely upon the veracity of my informant, who had been for many years a sufferer [from chronic hæmorrhoidal bleeding]. His fees to medical men had quite impoverished him, and ultimately rendered it necessary for him to obtain the hospitable shelter of the Charter House, where he ended his days at the age of eighty-seven. In

giving me the history of his case, he told me that he had been a sufferer for twenty years from an almost daily discharge from his bowels, which kept him so weak that he was scarcely capable of attending to his business. He was a glover by trade. He had consulted all the best medical men and surgeons of his day, and amongst the latter, I remember, were Sir Astley Cooper and Chevalier. No one did him any good. An old woman came into his shop one day, and addressed him thus:—‘Mr. M——, you look very ill; would you mind telling me what is the matter with you?’ He felt no hesitation in doing so. And his visitor replied—‘I thought from your appearance you were suffering from a loss of blood, and that is why I asked you. If you will do as I tell you, I will cure you. Get a piece of alum the size of a pigeon’s egg, and carry it constantly in your breeches pocket, and I will call in a fortnight and ask how you are.’ She called at the time she promised, and Mr. M—— was able to inform her that at the end of a week the bleeding had stopped, and he had had no return of it. This recital appeared to me as strange as the belief that many people formerly entertained of the efficacy of carrying a cramp bone in the pocket for the prevention of cramp; but although I felt incredulity, I expressed none, as I knew that my informant would relate only what he believed to be true. I asked him how long it was since he commenced to act on the old woman’s advice. He replied, ‘twenty years, and I have never had any return of the bleeding.’ I inquired whether the alum that he carried in his pocket diminished in size. He said that it wore away after a few weeks, and he had occasion to renew it constantly. He put his hand into his pocket and produced a piece of alum that he had purchased only a day before. When I learned that the alum wore away, I was not so surprised as at first, and an explanation of the *modus operandi* of so simple a remedy occurred to me. The heat and moisture of the body had acted upon the alum, and caused it to give off its astringent property, which, being absorbed by the skin, exerted its influence upon the pelvic viscera and blood-vessels. The effect was to constrict the hæmorrhoidal vessels and to put a stop to the flow of blood. I have tried this simple remedy in several instances, and usually with success. When ordered for females, I make them wear a piece of alum in a small packet suspended from the waist; and I have some interesting daily records of the diminution of the flow of blood from the bowels, when the cases have been suitably selected.”

So, too, in suitably selected cases, Hahnemann “cured” patients by giving them glass bottles containing drugs to hold in their hands.

We shall notice but two or three other points in the remaining chapters of Dr. Hood’s book. Rheumatism, as well as gout, he attributes ultimately to functional derangements of the liver, an

infers that treatment should be directed to restoring healthy hepatic action. Calomel, in small doses, combined with James' powder, colocynth, and hyoscyamus, is his remedy. Sciatica is, he thinks, traceable to the same origin. The use of colchicum in the treatment of acute sciatica is as effective in relieving pain, and as mischievous in its results as in gout. In those cases of acute rheumatism, only, in which the eye is engaged is the exhibition of colchicum justifiable. In conclusion, let us offer a consolation, on Dr. Hood's authority, to those who indulge in after-dinner naps. The tendency is natural and "as such may be supposed to be beneficial." Dogs do it. Let "all who feel a similar impulse yield to it if they are able," fearless of the apoplexy threatened by vulgar error.

It is a pity that provincial printing and careless "reading" have allowed this most amusing and instructive book to be disfigured by innumerable misprints.

3. The third volume on our list professes to record the experience of a Brighton physician. Dr. Roose strikes the key-note of his composition in his preface. He is "fully convinced that functional disorder of the liver underlies the majority of gouty manifestations, and that the kidneys are only secondarily implicated." In his second chapter (p. 21) will be found a concise summary of all that is known about uric acid; its quantity in health and disease, and the effects of ingestion of water, of alcohol, and of various drugs. As to *where* it is produced opinions differ—some authorities (including Dr. Roose) holding that it is formed throughout the body, wherever disintegration of tissue is going on, and merely discharged through the kidneys; others, that it is produced in the kidneys only. Then, uric acid being the admitted proximate cause of gout, another question is discussed:—Is the disease due to retention of the acid in the system, in consequence of deficient eliminating power in the kidneys, as taught by Dr. Garrod? or, to increased production from suboxidation of the tissues or other cause? The third chapter is devoted to the support of the latter view, and to proving that the liver is the principal source of over-production. Of the three hepatic functions—the secretion of bile, the formation of glycogen, and "the destruction of albuminous matters derived from the food and textures, and the formation of urea and uric acid"—it is the last with which the origin of gout is connected. Derangement of this function, leading to "imperfect transformation

of albuminous matters," and the production of uric acid instead of urea may well lead to the lithæmic dyscrasia. The neurotic origin of gout and Dr. Meldon's views—which Dr. Roose calls "a neuro-humoral theory"—are discussed in the same chapter which concludes with a summary of his conclusions, which we give:—

"1. Uric acid, in the form of sodium urate, is the *materies morbi* of gout. 2. The deposit of sodium urate in the joints is the cause of the gouty inflammation. 3. This substance is produced in excess, as a result of the imperfect transformation of albuminous substances. 4. This imperfect transformation is for the most part due to functional disorder of the liver, or to excessive supply of nutritive materials, or, as often happens, to a combination of these causes. 5. So long as the excess of uric acid is eliminated by the kidneys, decided attacks of gout may be absent; but the symptoms above described as pertaining to the uric acid diathesis are liable to be present. 6. The kidneys are apt to become *secondarily* affected, owing to the irritation set up by excess of uric acid and other products of defective metamorphosis, and by deposits of urates. Primary disorder of the kidney is not a necessary factor in the production of gout. 7. In the majority of cases of chronic gout increased production of uric acid is associated with defective elimination by the kidneys. 8. The symptoms of nervous disorder in gout are due to the action of the *materia peccans* on the nerve-centres" (p. 57).

Of Dr. Roose's concluding chapter, on "Treatment," we shall mention only a few points, corresponding to those specially noticed in our remarks upon the other works reviewed. Meat is not forbidden, but restricted in amount. Carbo-hydrates are to be sparingly used, *sugar* and starch especially. A little fruit is allowed "provided that no undue amount of acid or of saccharine materials is thereby introduced into the system." *Alcohol*, in any form, is forbidden; except in small quantity when digestion is feeble. "Imperfectly fermented and effervescing wines, and malt liquors of all kinds, should be strictly forbidden." As to drugs, Dr. Roose, in the acute attack, flies to the colchicum-bottle at once, except "for weakly subjects." In the intervals he recommends *inter alia* iodide of potassium, especially when lead-poisoning is also present. The skin affections require constitutional treatment, local treatment being of subordinate importance.

It will be seen that we have not yet arrived at unanimity in the treatment of *nodosa podagra*.

The popular demand which exists at present for a reliable work on gout is sufficiently evidenced by the fact that a second edition



of Dr. Roose's work has been called for within six months. In the foregoing references to the pages from which quotations are made, we have used the second edition, which has only just issued from the press.

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*Lectures on Dietetics and Dyspepsia.* Delivered at the Owens College School of Medicine in February and March, 1885. By WILLIAM ROBERTS, M.D., F.R.S. London: Smith, Elder & Co. 1885. Small 8vo. Pp. 92.

THE first of these lectures is of a general character, and treats of dietetics, the origin of food customs, and the effects on the nutrition and character of different kinds of feeding. It is full of suggestive and interesting matter, of which we can reproduce only a few examples.

The greater frequency of digestive troubles among men than among animals is to be found not in a difference in the proximate principles of the food in the two cases, not in a difference in the nature of the digestive secretions, but in the "quicker and more universal sympathies of his nervous system, and partly and chiefly in the extraordinary complexities which civilised man has introduced into his dietary." Not only are the articles of diet various, but further complexity is introduced by the varieties in cooking, and, above all, in the condiments and articles of a restorative and stimulating character—as tea, alcohol, tobacco—which are so largely partaken of by man.

Except for those who, from idiosyncrasy or want of power of restraint, are unable to take alcohol in moderation, and who find it easier "to abstain than to be abstemious," Dr. (now Sir William) Roberts is in favour of the use of alcohol, and believes that its universal consumption by all the progressive races shows that it subserves some useful purpose in the economy. He shows that the English do not deserve the bad character they have got for the abuse of alcohol, but that while individual excess is more common in England than on the Continent, the consumption of alcohol per head is pretty much the same in all civilised countries.

On the use of tea, coffee, cocoa, the following interesting suggestion is made:—The employment of these substances has become so large and universal within the last hundred years that we may now form a good judgment as to their effect on the national character.

That this effect has not been injurious is shown by the continual progress which the nations of Europe have made :—

“Reflecting on this matter, I have not been able to avoid the impression that it is possible to trace a change in the mental type of the Western races in the last three generations. There is, I think, to be observed an increased precision in their mental operations, resulting in an improved criticism, and in the rise and progress of the exact sciences. It is certainly remarkable that within the last century, coincident with the spread of tea and coffee and cocoa, and, perhaps I should add, of tobacco, and in combination with the ancient use of alcohol, there has been, within this brief epoch, more progress made in criticism and the exact sciences, and their dependent industrial arts, than in all the preceding ages of this world; whereas, during the same epoch, art and literature, which depend more on the imagination, have practically stood still.”

We cannot but think, however, that the value of this suggestion is somewhat impaired by the well-known fact pointed out in the next page—namely, that while men consume four-fifths of all the alcohol used, women, in whom the imagination is supposed to override the reason, consume the bulk of the tea and coffee.

Of the variations of diet suitable for the different ages and sexes, and on the effect of different kinds of food on the nutrition and vital habits, we find many valuable and suggestive remarks. These are illustrated by facts observed among the lower animals, and—transferring to men the terms high and low feeding applied to dogs and horses—

“It may be said that high feeding, in the case of man, consists mainly in a liberal allowance of meat and in the systematic use of alcoholic beverages, and that low feeding consists in a diet which is mainly vegetarian and non-alcoholic. . . . The high-fed classes and races display on the whole a richer vitality, more momentum and individuality of character, and a greater brain-power than their low-fed brethren, and they constitute the soil, or breeding ground, out of which eminent men chiefly arise.”

In the second lecture are given the results of *experiments on the effect of food accessories on salivary digestion.*

The great richness in diastase of human saliva is supposed to be in special relation to the habit man has acquired of cooking his food, and so of making the starchy matter accessible to the action of this ferment.

The experiments were made by taking a standard solution of pure

potato starch, mixed with a certain volume of saliva, and determining the time which elapsed before the mixture ceased to give a blue or red colour with iodine. This was found very constant, and was called the "achromic point." Then a similar mixture of starch and saliva, together with the substance to be experimented on, the bulk of the whole mixture being the same as before, was examined, and the time required to reach the achromic point was determined.

*Alcoholic Beverages.*—The addition of up to 20 per cent. proof spirit did not affect the digestion of starch, and even with 90 per cent. there was some action. Gin gave approximately the same results as proof spirit; French brandy, 10 under proof, in the proportion of 10 per cent., greatly retarded the change of starch, and in higher proportion completely stopped it; Scotch whisky, marked proof, was nearly as bad, and above 20 per cent. there was no action. The effect of brandy and whisky was, then, due not to the alcohol, or to the acid—since neutralisation did not alter their effect—but to the ethereal bodies and volatile oils, and, in the case of brandy, to tannin. It is, however, pointed out that the small quantity of alcohol usually taken with food, not above 5 per cent., can have no injurious effect, but must rather promote digestion by increasing the flow of saliva.

*Wines*, both strong and weak, exerted a powerful inhibitory action on salivary digestion. Even 0.5 per cent. of sherry or hock retarded, and 1 per cent. almost completely stopped, the change of starch by saliva. Port and claret were as bad. This is due altogether to the acid, for when this was neutralised the inhibitory power was lost.

*Acids.*—Even 0.02 per cent. (1 in 5000) of vinegar retarded digestion of starch, and 0.2 (1 in 500) completely stopped it.

*Malt Liquors* hampered salivary digestion exactly in proportion to their degree of acidity.

*Effervescent Table Waters.*—Waters simply charged with carbonic acid exerted a very considerable inhibitory influence on the action of saliva, but those which contain alkaline carbonates did not. The admixture of these alkaline waters with wine, as is so commonly practised, is advantageous.

*Tea, Coffee, and Cocoa.*—Tea (5 per cent. strength, infused for ten minutes, and filtered) retarded salivary digestion, even when only 1 per cent. was added to the mixture of starch and saliva. Above 10 per cent. the action was completely stopped. This

is due entirely to the tannin, and it was found to correspond to that exerted by a solution of tannin of equivalent strength. The popular idea that tea, when infused only for a short time, does not contain tannin is erroneous. The effects of tea were the same whether it was infused for two or for thirty minutes. Caffein was found to have no effect, and an infusion made from tea after exposure to heat, by which the volatile oil and the alkaloid were driven off, was found to have lost none of its inhibitory power. The injurious effect of tea may be completely prevented by adding to the infusion a small quantity of bicarbonate of sodium.

*Coffee* (5 per cent. strength) had far less effect than tea. Up to 40 per cent. there was no retarding influence observed, and even 60 per cent. did not very powerfully delay the appearance of the achromic point.

*Cocoa* may be regarded as practically indifferent. So may *beef-tea*, *salt*, and *sugar*, in the proportions in which they are taken in food.

The third and fourth lectures are devoted to an investigation of the effect of *food-accessories on peptic digestion*. The problem here is more complex than in the case of salivary digestion. The plan of experiment adopted was as follows:—Dried, finely powdered lean meat or fish, or finely divided boiled white of egg was placed in equal quantities in several tubes; to one, the control tube, was added 100 cc. dilute hydrochloric acid; to the others was added the same quantity of real acid, diluted partly with water, partly with the matter whose effect was to be studied, but in every case so as to make the volume 100 cc. The tubes were placed in a water bath, and after a time there was added to each an equal quantity of a powerful glycerine pepsin extract. The time required for the solution of the proteid was noted and compared with that in the case of the control tube.

The effects of varying acidity and variation in quantity of pepsin were first investigated. It was found that with from 0.1 to 0.3 per cent. HCl. the rapidity of digestion was practically the same, but beyond these limits it was much delayed; the best results were got with a strength of 0.2 per cent. The more pepsin present the more rapid the action.

*Proof Spirit*, *Brandy*, *Whisky*, *Gin*, acted simply in proportion to the alcohol they contained. When added to the digestive mixture in less proportion than 10 per cent. they had no effect,

and until the proportion passed 20 per cent. the effect was very slight. Above this, however, the effect was marked. Hence, taken in moderation, these beverages can have no retarding effect on digestion, but must promote this process by increasing the flow of gastric juice.

*Sherry and Port Wines.*—The effect of these was altogether out of proportion to the alcohol present. Even 10 per cent. delayed and 40 per cent. completely stopped peptic digestion. Hence, these wines, as ordinarily drunk, must often exert a very considerable effect in retarding stomach digestion.

*Hock, Claret, Champagne,* exerted a retarding influence out of proportion to their alcohol. Forty per cent. caused considerable delay. Champagne has less effect than hock or claret; in small doses, 10 per cent., it even favours digestion, probably by the mechanical effect of the escaping gas; in the large quantities often consumed these wines may exert a decidedly retarding effect on digestion.

*Malt Liquors,* in proportion of 20–40 per cent., exert a decidedly retarding influence, which is, of course, not due to the alcohol. Their effect must often be exerted in the stomach.

*Effervescent Table Waters.*—Simple aerated waters, in proportions of from 10–40 per cent., favour digestion by the stirring up of the mixture caused by the escaping gas. The alkaline carbonated waters, even up to 90 per cent., exerted very little retarding effect, and in smaller doses were rather advantageous.

*Tea and Coffee* both exerted a powerful retarding effect on peptic digestion. This is in proportion to the strength of the infusion, which is usually greater in the case of coffee. Cocoa in equal strength has the same effect as coffee.

*Beef-tea and Whey* both retarded digestion, the former equal to 5 per cent. tea, the latter less—about equal to hock.

Cane sugar, glycerine, and decoctions of fruits, except in very large proportions, had no effect.

The explanation of these results cannot be completely given, but there are certain points which have been ascertained and which throw much light on the subject.

In the case of beef-tea and whey the effect is due to the salts of the organic acids, lactic and sarcolactic acid, present. These are decomposed, and, while the organic acid is liberated, the more powerful hydrochloric acid is combined to form salts. It is shown experimentally that the addition of even a very small quantity of

tartrate of potassium greatly delays the process of peptic digestion, which is also much impeded by the presence of alkaline chlorides. That this explanation is the true one is further shown by the fact that if the quantity of hydrochloric acid in the mixture is increased, the retarding effect of whey and beef-tea is, to a great extent, neutralised. Superacidulation was found to have considerable effect in obviating the ill effects of coffee, less of tea, and none of light wines or Burton ale, while it distinctly increased the inhibitory action of sherry and port wines.

When the food accessories were submitted to dialysis for six hours, and in this way freed from some of their crystalloids, it was found that the retarding effect of beef-tea and whey was greatly diminished, that of coffee and ale less, that of tea and light wines much less. Dialysis for twenty-four hours completely removed the retarding action of all these substances. From all this it appears that the retarding action of whey and beef-tea is due to the saline matter, that of coffee partly to salts, partly to cafeeo-tannin; that of ale to saline matters, which, since the effect is not removed by superacidulation, are probably inorganic salts, phosphates, and chlorides. Direct experiment showed that the alkaloid of tea and coffee and the volatile oil were without effect. The tannin in tea accounts for one half of its retarding effect. In the case of wine an explanation cannot be fully given. Boiling for five minutes, so as to expel the volatile ingredients, greatly diminished its inhibitory action.

Sir Wm. Roberts argues that the retarding effect exerted by all these common articles of diet on the process of digestion in the stomach is not an evil, but subserves a useful purpose in preventing the products of digestion from being thrown too rapidly into the system, where they might exert an injurious action, and be hurried out of the body by the excretory organs before they had time to be assimilated.

The food accessories can exert but little influence on pancreatic digestion, owing to their change and absorption in the stomach, and to the altered reaction in the duodenum:—"It may, therefore, be concluded that, with regard to pancreatic digestion, the effects of food accessories are practically *nil*." This conclusion is supported by numerous experiments.

In framing a dietary it is necessary to distinguish between gastric and intestinal digestion. In healthy persons and those slightly ill the former has chiefly to be taken into account, but in

those who are seriously ill the stomach ceases to act, and serves merely as a passage for matters into the duodenum. In these cases milk is, as experience has shown, the best diet, containing as it does all the elements of nutrition, and being of all foods that which is best digested by the pancreas :—

“In feeding the sick, our first consideration should be whether we are aiming at feeding the stomach or feeding the duodenum. In the former case, when the patient can take solid food—and this is the diagnostic indication that the stomach still possesses digestive activity—our aim must be to administer meat, bread, eggs, &c., in a state most favourable for peptic digestion. The meat should be well cooked—by preference boiled. It should be finely comminuted, either by perfect mastication in the mouth, or (if this be impossible) by pounding in a mortar, or beating to a paste with a spoon, as in the preparation of potted meat. Beef-tea and soups should be used sparingly, as should be likewise tea, coffee, and alcoholic beverages. And of these last the best adapted for weak stomachs are regulated quantities of ardent spirits or of the stronger wines or champagne.

“On the other hand, when we are aiming at feeding the duodenum, our chief reliance, as I have said, must be on milk, which may be administered in a score of ways, with tea, coffee, cocoa, spirits, soups, gruels, &c. Alternately with the various preparations of milk may be administered meat-teas, beaten-up eggs, cold-made meat infusions, and jellies.”

The last lecture is on “The Acid Dyspepsia of Healthy Persons.” This “consists essentially in a tendency or predisposition of an enduring character, the tendency being towards a generation or accumulation of excessive acid in the later stages of gastric digestion.” The digestion is not slow, on the contrary often unduly rapid; but, after the process of digestion proper is over, there remains a residuum of acid mucus mixed with remnants of food, chiefly of a fatty nature. The acid consists of hydrochloric, together with organic acids, as lactic and butyric. These latter are not produced by fermentation, but are liberated from the food by the excessive hydrochloric acid secreted by the stomach. The degree of acidity of the residuum far exceeds that of the normal stomach contents. The symptoms of this form of dyspepsia are—pain, depression, acid eructations and heart-burn, flatulence and gastric cramp, or paroxysmal pyrosis. Sir Wm. Roberts thinks the pain and distress account sufficiently for the depression without calling in the aid of any poisonous alkaloid generated during digestion. The flatulence

is due not to gaseous products of fermentation but to swallowed air, which, owing to the increased salivation, is greater in amount than under normal circumstances, and also to carbonic acid liberated from the carbonates of the swallowed saliva. Of the symptoms of gastric cramp, or paroxymal pyrosis, a most graphic description is given, and it is shown that the gush of fluid into the mouth, which is often supposed to be due to regurgitation from the stomach, is really a sudden and exaggerated flow of saliva. The whole phenomenon is looked on as an abortive act of vomiting.

The diagnosis of acid dyspepsia, often difficult, may be assisted by testing the effects of a dose of alkali. If this does not relieve the symptoms, then the affection is not true acid dyspepsia. As to the treatment, Sir Wm. Roberts suspects that the prolonged use of alkaline drugs has an injurious effect on the system, and tends to cause calcareous degeneration of the blood vessels. He proposes to get the alkali for the neutralisation of the gastric acid from the system itself by increasing the flow of saliva. He has found much benefit from the use of lozenges of either simple gum, or of gum mixed with some stimulating substance, as ginger, cayenne-pepper, or pyrethrum. We may add that Sir Wm. Roberts suffers himself from acid dyspepsia, and many of his observations of this interesting affection have been made on his own case.

We have not hesitated to give a lengthy abstract of this most interesting work, but even a much longer notice would fail to convey the charm of style of the lectures and the fulness of suggestive matter, both physiological and clinical, with which every page abounds. We can only say that this volume is a worthy successor to the author's lectures on the digestive ferments. We could scarcely give it higher praise.

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*The Influence of the Sympathetic on Disease.* By EDWARD LONG FOX, M.D., Oxon., F.R.C.P. London: Smith, Elder, & Co. 1885. 8vo. Pp. 565.

WE confess that we approach a work on the sympathetic system of nerves with considerable misgivings. This part of the nervous system has always been the happy hunting ground of medical theorists, and there is no disease or morbid symptom which has not been attributed to "an influence of the sympathetic." If we could have it certainly established that the sympathetic ganglia and nerves ever act independently of the central nervous system—



if some unquestionable examples could be adduced of even reflex action taking place through a sympathetic ganglion—something would be gained; but when we so often read that in one disease the semilunar ganglion has been found reddened, in another that the inferior cervical ganglion was enlarged, and in a third that pigmentation was observed in some other part of the nervous system, and when from these isolated and often badly observed cases far-reaching pathological conclusions are drawn, it cannot be wondered that we feel rather a dread of the influence of the sympathetic. Furthermore, medical writers on the sympathetic seem to be unable to realise that vaso-motor paralysis does not, *per se*, materially influence the nutrition of parts, as any one who has divided the cervical sympathetic in a rabbit knows, and that the secretion of a gland is not necessarily called out by a dilatation of its vessels. These points are admitted by these writers in their chapters on the physiology of the sympathetic, but invariably forgotten when they come to speak of the diseases supposed to be due to this nerve.

We regret to say that Dr. Fox's work, while evincing great literary labour, does not diminish our distrust of the sympathetic. He says in his preface:—"The three most important points that the consideration of the sympathetic seems to me to illustrate are—(1) the marvellous effect of reflex action in health and disease in connection with this system; (2) the exquisite mutual dependence of the various portions of the nervous system—cerebral, spinal, and sympathetic—on each other; (3) the fact that, in spite of this mutual dependence, the sympathetic, under conditions of brain or cord, is enabled within certain limits to act independently." But surely it is not necessary to write a volume of 600 pages to illustrate these points, which must be familiar to every one who has read even a student's text-book of physiology.

The work begins as usual with chapters on the anatomy and physiology of the sympathetic. These are followed by a short chapter on the general pathology, and then, in a series of chapters, we have the special pathology, or an account of the various diseases supposed to be caused or influenced by the sympathetic nerve. It would be of course impossible for us to follow the author through all of these, but we shall take a few passages at random which will give some idea of the nature of his work.

Speaking of the different conditions which cause pain in the stomach, he says (p. 387):—"Pyrosis is a pure neurosis, connected

only with the vaso-motor system. It is a result of a paretic condition of the vaso-motors, and it is a matter of doubt whether the fluid itself is a cause of gastralgia, or whether both phenomena are the consequences of one and the same defect of innervation." Now we ask what does a paretic condition of the vaso-motors mean? Does it mean that the nerves cannot convey impulses, or that the ganglia from which vaso-motor impulses are sent out, and which lie not in the sympathetic but in the brain and cord, are no longer able to send out these impulses? How does the paresis of the vaso-motors cause either the pain in the stomach or the fluid, which in many cases comes from the salivary glands? To these questions we find no answer.

In the chapter on myxœdema we find mucin spoken of as a *fœtal tissue*. Again we read—"Mucin is the ground substance of connective tissue, not the fibrous element. Morochowitz teaches by his researches that all the tissues of the connective tissue type have a common chemical origin in the embryo from collogon (*sic*) (which yields gelatine on boiling) mixed with mucin. In myxœdema there is probably a retrograde metamorphosis." We do not think we can be accused of hypercriticism if we look on this passage as obscure. If mucin be a tissue, how can it be the ground substance of a tissue? There is no doubt mucin, which is a chemical substance and not a tissue, can be extracted by suitable means from fibrous connective tissue, as shown by Rollett, but it does not constitute the whole of the ground substance, and the exact position it takes in the interfibrillar substance is not certainly known. And what is meant by the retrograde metamorphosis? What retrogrades? and what is metamorphosed into what?

Finally, in the last sentence in the work we find the following summary of the author's views on the pathology of a very obscure disease, scleroderma:—"It suffices to say, that the *post-mortem* appearances, that account for most of the phenomena are not themselves the essence of the disease, but the results of the primary lesion; that this primary lesion is vaso-motor paralysis (1); and that the condition of the vaso-motors is the precise opposite of what is met with in symmetrical gangrene of the extremities, and that this paretic state of vaso-motors may be induced either by a traumatic lesion, by the effect of certain constitutional states on the vaso-motor centre—states like anæmia, rheumatism, &c.—or sometimes by definite injury to important vaso-motor centres, as by sclerosis of the anterior columns of the spinal cord."

This example of sympathetic pathology we may present to our readers without comment.

The work, which is handsomely printed and brought out, is illustrated by some rather rough engravings and by three coloured plates representing microscopic appearances. We defy anyone seeing these figures, without the explanation, to know what they are meant for. The third plate, representing the skin in scleroderma, is more like a map of the moon than any section of skin we have ever seen.

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*Year-Book of the Scientific and Learned Societies of Great Britain and Ireland, comprising Lists of the Papers read during 1884 before Societies engaged in Fourteen Departments of Research, with the Names of their Authors.* Compiled from Official Sources. Second Annual Issue. London: Charles Griffin & Co. 1885. Pp. 231.

THE nature of this useful compilation is amply explained by the title-page. We gladly recommend it to everyone interested in scientific pursuits. The first issue of the Year-Book, for 1884, contained an authentic account of the "history, organisation, and conditions of membership" of the Societies. Future issues—and we trust they may be many—will resemble the present one.

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*Handbook of General Therapeutics.* In Seven Volumes. Vol. I.—*General Introduction*, by DR. VON ZIEMSEN. *On the Dietary of the Sick, and Dietetic Methods of Treatment*, by PROFESSOR J. BAUER. *On the Koumiss Cure*, by DR. STANGE. Translated by EDWARD F. WILLOUGHBY, M.B. London: Smith, Elder, & Co. 1885. Pp. 408.

THE success the English translation of Prof. von Ziemssen's "Handbook of Special Pathology and Therapeutics" has secured has been deservedly great; and its teachings have had, we believe, a beneficial effect on English and Irish clinical and pathological observation and work. There is little reason, therefore, to doubt but that the translation of another work of the same class, written also by eminent specialists selected by the same experienced editor, and which in the original has received the warmest encomiums, will meet with equal success. As was the original plan, the two great works form a single whole. The one series is the complement

of the other, and now that the "Handbook of General Therapeutics" has been published, and von Ziemssen's great double work thus brought to so successful a completion, we may heartily offer him, and German medicine generally, our congratulations on the achievement.

Of the English translation four volumes have already appeared. So far the publishers have to be commended upon having secured as translators gentlemen so competent from their professional status, as well as their knowledge of the German language, as Dr. Matthew Hay, Dr. Burney Yeo, Dr Willoughby, Dr. Port, and Dr. Macpherson. Most of these gentlemen are known to have given special attention to the subjects they translate, and all seem to have done their work—the difficult work of translating German, bristling as these volumes do with technical words and expressions not ordinarily used in medical literature—remarkably well and correctly.

Although the lengthy details and numerous tables met with in this great system of therapeutics in many cases are given with such fulness and minuteness as almost to weary the reader, it must be remembered that the work is an encyclopædic and standard one. It is written for the well-educated and scientific physician, and its high object is to establish scientific therapeutics on a positive and firm basis of facts, by bringing together as far as possible requisite scientific principles, and the strictly scientific results obtained by the experimental method of research, as opposed to empiricism.

The four volumes already published comprise, in addition to the first volume, complete monographs by acknowledged experts. Thus Antipyretic and Antiphlogistic Methods of Treatment, and the Hypodermic and other Methods of Administering Medicines Cutaneously by Liebermeister, Jürgensen, and Eulenberg, constitute Vol. II. Vol. III. is by Professor Oertel on Respiratory Therapeutics; and Vol. IV. (the Treatment of Disease by Climate, and General Balneotherapeutics) is by Hermann Weber and Professor Lichtenstern. Other volumes upon General Orthopædics, Gymnastics and Massage; Hydrotherapeutics; the Therapeutics of Circulatory Derangements; and an important and valuable Manual of Electro-Therapeutics by Erb, will shortly follow.

The greater portion of the first volume is by Professor Bauer, who discusses under various heads, and in an exhaustive manner, the subject of dietetics in disease, and dietetic methods of treatment. He first indicates the value of the more important articles

of food, and of such substances as sugar, alcohol, tea, coffee, vinegar, and other condiments. Considerable space is devoted under this head to the subject of metabolism, and the value of peptones as nutriments for the sick, about which more information is now available than when the author, some ten years ago, so correctly indicated the advantages that would probably result from their use in the future. Numerous tables, chiefly quoted from J. König, are given, showing the composition of the different kinds of food. Due stress is laid upon the importance of the proper and careful preparation and cooking of food for the sick. As regards beef-tea, meat extracts, peptones, &c., Professor Bauer prefers the meat solution prepared by Leube to every other similar preparation in the market. He points out that although all such preparations contain but very small amounts of actual nutriment, the recognition of this fact in no way lessens their value in the dietary of the sick. "One must bear in mind that during the exclusive administration of broths the organism receives only stimulants, and starves for want of food. So soon as a desire for actual food presents itself broths are no longer sufficient; they are, however, very well adapted for the preparation of really nutritious soups, in which the most diverse kinds of nutriment may be provided, and the advantage of an agreeable flavour may generally be combined with the fine division of the nutritive materials."

The chapter on the Digestion and Utilisation of Nutriment deals chiefly with the physiology of digestion and the actions of the digestive secretions. The effect of fever in arresting the gastric secretion, and the consequences of a deficiency of the normal acid in it, are clearly pointed out, as are also the circumstances which hinder the passage of the chyme into the duodenum—a less-frequently recognised cause of indigestion. The colour of the fæces in jaundice is stated to be due rather to the presence of a large quantity of fat, than to the absence of biliary pigment, as commonly supposed. The knowledge which this chapter seeks to convey, and which is so desirable and necessary in the rational dieting of the sick—viz., how much of any given form of nourishment is absorbed, and how much passes through the bowel unused—is still, however, in its infancy.

The Demand of the Organism for Nutriment is considered under the various circumstances under which it is necessary to determine the amount of food adequate to the requirements of man in health and in disease. Like the other sections of the work this also is

prefaced by a copious bibliography. It is mentioned here and elsewhere in the work that cow's milk does not constitute a sufficient diet for the sole food of healthy adults, since it contains too small a proportion of non-nitrogenous bodies relatively to the albumen present. The importance of this in cases of disease is obvious, for if a person suffering from a chronic fever takes large quantities of milk, he receives a highly albuminous diet, and one that under normal circumstances would call for a further addition of fat and carbo-hydrates.

The most interesting and useful divisions of the work to the practitioner are those on Special Dietetics for the Sick, and Dietetic Methods of Treatment. Under these heads will be found valuable information on the administration of food in acute and in chronic fevers, in convalescence, in diseases of the digestive organs (including artificial feeding), and in special diseases, *e.g.*, anæmia, scrofula, rickets, gout, diabetes mellitus, and in scurvy. Special dietetic methods of treatment, such as Bantingism, Vegetarianism, the Dry Cure, the Grape Cure, the Milk and Whey Cure, are also scientifically considered, and their indications, mode of employment, and value fully explained.

Although there is necessarily, perhaps, a great deal of dry reading in this work, it is one of great value, and without an equal of its kind in our language. There is no physician who has read it, we think, that will not be glad to have it by him for reference. We may say the same of all the other volumes that have as yet been issued, but further notice of which at present we are obliged to defer.

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*The Extra Pharmacopœia.* By WILLIAM MARTINDALE, F.C.S., and W. WYNN WESTCOTT, M.B., Lond. Fourth Edition. London: H. K. Lewis, 1885. Pp. 416.

THIS admirable little work is now so well and so favourably known that it is merely necessary to draw attention to the appearance of the fourth edition, which has, in every way, been brought up to date. The authors have added about one-fourth more matter, but in so condensed a form that, in outward appearance, shape, and size, the present edition most closely resembles its predecessors.

The book now opens with a succinct review of the British Pharmacopœia of 1885—its additions, omissions, changes in nomenclature, and principal alterations. And in this connection we would

particularly mention with approval the authors' criticism of the new edition of the *Pharmacopœia*, which will be found at page 27. They comment unfavourably on the omission of *mistura gentianæ*, *stramonium* leaves, and the green iodide of mercury.

There is a novel feature in the present edition which will enhance the value of the book as a work of reference. We allude to the secondary list of drugs to which the attention of the profession has been of late more or less directed, but which have not as yet come into general use.

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*The Journal of Microscopy and Natural Science: the Journal of the Postal Microscopical Society.* Edited by ALFRED ALLEN. Oct., 1885. London: Baillière, Tindall, & Cox.

THIS journal, published quarterly, has reached the conclusion of a fourth volume. It seems to be intended for the use of amateur microscopists and naturalists, and judging from the contents of the number before us, and from the index of the fourth volume, it appears to be well conducted, and to contain much interesting matter. The present number contains three longish articles—1. How Plants Grow, by H. W. S. Worsley-Benison; 2. The Microscope, and How to Use it, Part IV., by V. C. Latham, composed entirely of receipts for staining fluids; 3. Pond Life, by W. E. Hoyle. These are followed by a number of short articles, comprised under the title—Half-an-hour at the Microscope with Mr. Tuffen West. These are illustrated by six fairly executed plates. Then come Notes from the Note-books of the Members of the Postal Microscopical Society; and finally a number of short notices of books. On the whole this seems to us a good journal, and full of varied and instructive reading. The price is only one shilling and sixpence per number.

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*Practical Histology and Pathology.* By HENEAGE GIBBES, M.D. Third Edition. London: H. K. Lewis. 1885. Pp. 196.

THIS work has evidently met a want, since within a short time it has reached a third edition. The present issue is considerably enlarged and thoroughly revised, while the plan of the former editions is preserved. It will be found a useful guide by those who are commencing to work at the microscope without the aid of a teacher.

## PART III.

### MEDICAL MISCELLANY.

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*Reports, Transactions, and Scientific Intelligence.*

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#### ACADEMY OF MEDICINE IN IRELAND.

President—ROBERT M'DONNELL, M.D., F.R.S.

General Secretary—W. THOMSON, M.D.

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#### PATHOLOGICAL SECTION.

President—T. EVELYN LITTLE, M.D.

Sectional Secretary—J. B. STORY, M.B.

*Friday, November 6, 1885.*

The PRESIDENT in the Chair.

#### *Herpes Zoster.*

DR. C. F. MOORE showed an interesting case of herpes zoster.

#### *The Pathology of Lead Paralysis.*

DR. WALLACE BEATTY read a paper on the pathology of lead paralysis. Having described the lesions which have been found by different observers in the spinal cord, nerves, and muscles, and having discussed the theories which have been put forward to account for the paralysis, he exhibited sections of the spinal cord of a man, aged thirty-one, a painter, who was admitted into the Adelaide Hospital, under his care, suffering from lead paralysis. The man died of uræmia. There appears to be a marked change in the internal and anterior group of ganglion cells of the anterior cornua in both the cervical and lumbar enlargements, especially in the latter. The cells are both fewer in number—some sections showing only one or two—and smaller than natural; some stain badly, and in many the processes are not distinct. The ganglion cells of the lateral groups are well represented and appear quite normal. Round the central canal in both the cervical and lumbar enlargements



there is a large collection of cells, especially abundant in the lumbar region. The number of these cells appears to be considerably beyond the normal. No abnormality was found in the anterior nerve roots.

DR. HENRY KENNEDY said the late Dr. Todd found lead in the substance of the brain of a person who died from lead paralysis. As to particular nerves being selected for destruction, they knew that in the case of ordinary fatty paralysis that was exactly what occurred. If the sciatic nerve was touched the affection did not go continuously down, but attacked in patches.

PROFESSOR WALTER SMITH observed that Dr. Beatty's case belonged to a small section of a large chapter on muscular atrophy; and, though they could group certain forms of it tolerably satisfactorily, they were a long way from anything like a complete classification of those affections. The evidence, on the whole, pointed in the direction of central lesions, but he was not sure that they should not take into account lesions at both ends of the nerve systems. What they knew of the poisonous action of all the heavy metals pointed to that action taking place primarily at the nerve centres, the nerves themselves, and then the muscles and portions of the glandular apparatus.

PROFESSOR PURSER said it could not be affirmed that lead localised itself altogether in the centres of the nervous system. They found blue lines in gums which were due to sulphide of lead. There were facts showing that lead also localised itself in the muscular system. Experiments made some years ago by Professor Harnac showed that when an animal was poisoned by certain salts of lead which had not any action on the blood or tissues, but affected the system generally, the contractility of the muscles underwent a remarkable change.

DR. BEATTY briefly replied.

#### *A Case of Ulcerative Endocarditis.*

PROFESSOR PURSER made a communication upon a case of ulcerative endocarditis, the second he had seen. The patient, a man aged twenty-two, had had rheumatic fever three or four years previously, and had lately been very intemperate as to strong drink. On admission to hospital he was delirious, very violent, temperature  $104^{\circ}$ , pulse regular and strong, complained of pain in his head and ears. His heart and lungs were carefully examined and found normal. The only lesion visible on his person was a small recent scar on one knuckle, surrounded by loosened epidermis, as if a pustule had been present, which had burst before admission. There was no glandular enlargement in the arm, nor anything abnormal in its veins or lymphatics. On Oct. 24th a careful examination could detect nothing abnormal in either heart or lungs, but on the next day Prof. Purser discovered a loud systolic valvular bruit, which had certainly not been present on the 24th. This determined him

to the diagnosis of ulcerative endocarditis—a diagnosis which the subsequent *post mortem* completely established. The delirium and fever continued without much intermission till his death, which was preceded by œdema of the lungs, the bruit continuing all through. At the autopsy hæmorrhages were found in the brain, lungs, spleen, kidneys, intestines, and the retinæ. The heart and its valves were practically healthy, with the exception of the mitral valve, whose posterior curtain was ulcerated and completely disorganised, the surface of the ulcer being ragged, and covered with a diphtheritic-like deposit.

The microscope showed that this diphtheritic deposit, as well as the innumerable small hæmorrhages in the various organs (except those in the lungs, in which no micrococci could be detected), were caused by masses of micrococci, many beautiful specimens being shown at the meeting, where the vessels were visibly plugged by micrococci. Prof. Purser could find no other door of entrance into the body for the micrococci in this case than the small patch of abrasion on the knuckle. In the former case he had seen there had been suppuration of one of the vesiculæ seminales.

DR. MACSWINEY remarked that it had been already demonstrated by Dr. Osler, of Montreal, that in this remarkable affection the micrococci which formed the starting-point of the disease were conveyed to the heart by the blood, however they got into the latter. The left side of the heart was the one almost invariably attacked.

DR. FRAZER remarked that within the last year he had seen more curious and sudden developments of acute cardiac inflammation than in his whole life before.

PROFESSOR PURSER, in reply, said that during the first three days he was in the hospital the young man had positively no abnormal sound of the heart. The *post mortem* showed that the anterior curtain of the mitral valve was thickened, and that the chordæ tendinæ were thickened and shortened, but these conditions did not prevent the perfect closure of the valve. The walls of the heart were not thickened, but its weight was above what it should have been for the man's size. The walls of the aorta were rather thin, and the whole aorta itself small: and he thought at first that there might have been congenital narrowing of the aorta, giving rise to hypertrophy of the heart and disease of the valves. With that view he measured the diameter of the aorta, but found it not below what it should be. The only other explanation he could find of the increased bulk of the tissues of the heart was that in the sections from the kidneys he thought he could detect, in addition to the recent changes due to hæmorrhagic infarction, signs of local disease in the glands themselves—in fact he thought that the kidneys were in the early stage of Bright's disease. That would account for the increased size of the heart and the atheromatous disease, which was unusual

in a person so young. He did not think the valves of the heart were diseased before the acute affection occurred. They were thickened, but that thickening was not due to inflammation, but to the increased tension of the whole arterial system arising from the state of the kidneys. The affection of the valves was an acute affection, *ab initio*, of valves not in a perfectly normal condition, but at the same time not in an inflamed condition. They knew that diphtheritic affection of valves was not at all an uncommon secondary complication in acute endocarditis; but why the valves became affected they did not know. Some years ago an interesting paper was published by Professor Axel Key, in which he maintained that acute endocarditis was bacterial, and said that after acute endocarditis, or rheumatic fever, he had in all cases found bacteria in the valves. He also sought to establish that the way in which the bacteria reached the valves was not by circulating in the blood and then attaching themselves outside the valves, because, owing to the rush of blood, that was the last place which mechanical force would permit them to attach themselves to, but that they reached them through the blood-vessels, and he said that was the reason why the endocardial manifestations commonly occurred at the point where the valves met. In the present case he (Professor Purser) could see many of the blood-vessels of the valves completely filled up. Supposing that the micrococci entered through the young man's hand during the early days of his illness, while the high fever subsisted, they were circulating through the different parts of his body and plugging up the vessels; and after a certain number of days they excited inflammation and formed the fungous masses, and then the clinical manifestation of endocarditis occurred. This hypothesis would also allow them to suppose that the affections of the distant parts of the body were not so exclusively secondary to the disease of the heart as that the disease of the heart and of the other parts of the body were concomitant effects of the same cause.

#### *Zonular Cataracts and Dental Malformations.*

MR. STORY exhibited two patients with double zonular cataracts, and teeth presenting marks due to arrest of development. A cast was shown of the similarly deformed teeth of another patient, who also possessed double zonular cataracts; and a fourth patient was present whose teeth exhibited the same defects, but who had had complete soft cataracts in both eyes. The history was given of another case, in which double zonular cataracts and arrested development of the teeth had been observed. Mr. Story alluded to the work done by Arlt, Horner, and Hutchinson, and to the different theories proposed to explain the connection between zonular cataract and dental malformations. He drew attention to the close analogy between the development of the crystalline lens and that of a tooth, and gave it, as his opinion, that any cause whatsoever inter-

fering with the growth of the lens or of a tooth might produce the peculiar zonular cataract in the one, and the defects in the enamel of the other, which had been variously assigned to the action of convulsions, rickets, or mercury by different authorities.

PROFESSOR PURSER said there was one point on which Mr. Story's explanation rather limped. If layers of fibres were shaken into a state of degeneration, how could they generate fresh healthy fibres? Multiplication of lens fibres was the same as multiplication of any other epithelial cells. Another point was, that he did not think the nutritive material for the fibres of the lens went through these at all, but was conveyed through the intercellular substance. The theory of Arlt with respect to the fibres was rather mechanical, and did not commend itself to his mind, although it might be right. He thought the case an example of what was not an uncommon thing in the growth of epithelial structures—namely, a sort of rhythmical change. In a great number of animals the hairs were alternately of different colours; and rhythmical production of the same kind occurred even in the hair of human beings. In the mole the hairs were not cylindrical, but swelled at one point and then contracted and afterwards swelled again. Everyone had seen the white mark that occurs on human nails. These were usually quite irregular as to shape and the manner of their occurrence; but he himself had upon one of his nails a row of white spots which had been there for years, and which followed one another with perfect regularity along a single line. The alteration in the nutrition which produced them in that manner must be of a rhythmical kind.

DR. BENNETT said the question was—were the teeth in question rickety teeth?—because a great part of the communication depended on an alleged relation of zonular cataract to rickets. The teeth produced were supposed to be a connecting-link; but he would not call them “rickety” teeth, because the sign of the rickety affection was reversed in them. The rickety defect usually occurred at the base of the tooth, where the enamel met the cementum, and not at the cutting extremity. Very often an extreme amount of rickety disease occurred in patients without appearing in their teeth at all. He would be disposed to attribute the defect in the teeth produced to syphilis or mercury.

DR. HENRY KENNEDY was inclined to look on the whole disease exemplified in the case as the result of struma.

MR. A. BAKER remarked that dentists saw a large number of rickety teeth, and in very few cases did they find them accompanied with zonular cataract. He had seen a large number of cases of mercurial teeth, and had very seldom seen them connected with zonular cataract. The mercurial teeth were generally the six year old molars. The bicuspid teeth were generally free from this infection.

The PRESIDENT said the teeth now exhibited confirmed the observations

of Mr. Baker. The bicuspidæ were intact, while the front teeth had the mercurial characteristics.

MR. STORY, in reply, said he did not contend that the disease in the teeth exhibited was due to rickets alone, nor did he mean to assert that the peculiar form of cataract which he had brought under their notice was caused by rickets. It was acknowledged by Professor Horner and many other oculists that the form of cataract in question might be produced by various causes producing some arrest in the development of the lenses, whether these causes were rhythmical or otherwise. When these causes ceased to act the lens became clear, but if they were repeated the cataract recurred. He had himself seen in a human lens three distinct zones of opacity, which were caused at three different periods of the individual's life. He only used the term "ricketty" in reference to these teeth, because it was the earliest expression by which teeth of the kind had been described. Professor Horner had found that in 25 out of 36 cases of zonular cataract teeth of this sort were present; and they also existed in all the cases of zonular cataract that he (Mr. Story) had seen. He did not hold the mechanical shaking theory, but that there was some unknown interference with the development of the fibres. He agreed that the nutrition passed through the intercellular substance; but if the fibres of the lens became extensively degenerated it was hardly possible that the intercellular substance could escape disease.

The Section then adjourned.

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## SURGICAL SECTION.

President—SIR CHARLES A. CAMERON, M.D., President of the Royal College of Surgeons, Ireland.

Sectional Secretary—WILLIAM STOKES, F.R.C.S.I.

*Opening Meeting, Friday, November 13, 1885.*

The PRESIDENT in the Chair.

### *Inaugural Address.*

The PRESIDENT delivered an inaugural address dealing with the history of Irish surgery and the contributions made by Irish surgeons to surgical science. [It will be found in the number of the Journal for December, 1885. Vol. LXXX. No. 168, page 453.]

### *The Advantages of the Principle of Dry Dressings in Antiseptic Surgery.*

MR. KENDAL FRANKS read a paper on this subject. The ground for discussion on the principle of antiseptic surgery had been shifted lately.

That principle was almost universally recognised; the question now was as to the best methods of applying it. The ideal wound was one similar to a subcutaneous wound kept at perfect rest. The mode in which Lister's method failed to attain the ideal was that by the necessity of frequent dressings, absolute rest could not be secured. In order to get uniformly good results from the method of dry dressing, the following points were insisted on:—(1) The most rigid method of rendering and preserving a wound aseptic must be observed; (2) there must be absolute hæmostasis; (3) drainage tubes must be abolished, and in place of them a natural system of drainage substituted, by means of which no foreign bodies are left in the wound—a method due to Neuber of Kiel; (4) all cavities or spaces in the wound must be prevented by suturing together the deep tissues; (5) the application of large quantities of highly absorbent materials, rendered antiseptic; (6) the abolishing of all mackintosh or other impermeable material from the dressings; (7) the employment of a stable antiseptic in the dressings, instead of a volatile one like carbolic acid, or eucalyptol. The antiseptic best suited for the purpose is corrosive sublimate. The dressings employed by Mr. Kendal Franks consisted of a piece of sero-sublimate gauze previously wetted with carbolic solution—1 in 40—directly over the wound, and over the contiguous skin, so as to protect them from the action of the sublimate. Pads made of turf-moss containing 1 in 400 of corrosive sublimate, or wood-wool, containing 1 in 200 of the same antiseptic, made up in gauze bags, are applied over the deep dressing in sufficient quantity, and the whole firmly bandaged on with a calico bandage. These dressings can be left on for weeks. Mr. Franks gave some examples to illustrate the results obtained, which included cases of radical cure of hernia, healed under one dressing in ten days; excision of right lobe of thyroid gland, soundly healed in ten days under a single dressing; removal of cancer of the breast, with clearing out of axilla, under one dressing in a fortnight; excision of the knee, perfectly healed in three weeks under one dressing; and excision of the hip healed in three weeks under three dressings. These cases were not exceptional. Almost every operation case had a similar history; the cases quoted were only cited as examples.

There was no discussion; and

The Section adjourned.

# MEDICAL SECTION.

President—F. R. CRUISE, M.D.; President and Fellow, King and Queen's College of Physicians.

Sectional Secretary—A. N. MONTGOMERY, M.K.Q.C.P.

*Opening Meeting, Friday, November 20, 1885.*

The PRESIDENT in the Chair.

## *The President's Inaugural Remarks.*

THE PRESIDENT (DR. CRUISE) referred to the excellent and comprehensive work of last session, and hoped that of the coming session might equal, if not surpass it. The subject of cholera was of burning interest, and had been treated of last session in a paper of great ability. Although so fortunate as to escape a visitation of cholera this year, they knew not when it might arrive from the portions of Europe where it still lingered; and those who remembered the visitation of cholera in the autumn of 1866 would welcome any suggestions to combat so formidable an enemy.

## *Living Specimen.*

MR. ARTHUR BENSON exhibited a man who had an embolism of a branch of the central artery of the retina.

## *Specimens by Card.*

DR. DUFFEY showed the following:—1. A dilated and hypertrophied heart, with adherent and thickened pericardium and general endocarditis, from a boy, aged seventeen years. 2. Tubercular peritonitis. 3. General tuberculosis (?). Parts shown—(a) Large fatty liver. (b) Portion of thickened mesentery, showing deposit. (c) Portion of small intestine, showing deposit. (d) The cæcum and portion of large intestine, showing ulceration. (e) Heart and kidney, showing deposit on the latter. Patient was a female, aged fifty-two years.

*Some Suggestions on the Treatment of Cholera.* By JOHN R. BURKE, M.D.; Retired Deputy Inspector-General of Hospitals and Fleets, R.N.

After preliminary observations, the subject was divided into two parts—A. Prophylaxis; and B. Treatment.

A. *Prophylaxis*.—All sensible precautions of quarantine to be observed; hospital ships where strong current to sea, or hospitals on healthy situations on shore, with observation stations for those unaffected. Cargo of suspected ships to be disinfected or destroyed. Passengers' addresses to be registered. Special sanitary inspectors, with small districts and

magisterial powers; one member of each large institution to look after health of others; diarrhoea or malaise to be at once checked; frequent house-to-house visitations, at by no means regular intervals. Water sources to be jealously watched, and stopped if necessary, and condensers used for distilling; all water for drinking to be filtered through animal charcoal, boiled for some time, refiltered and slightly acidulated, or some Condry's fluid to be added; where water dangerous, condensers easily fitted. Street channels, sewers, markets, &c., to be flushed and disinfected. Rigorous food supervision. Purgatives or laxatives strictly forbidden. Thorough ventilation of houses. Disinfecting pits, chambers, or boilers, with pressure of 20 lbs., for every village (rough apparatus described). Complete cleanliness to be observed, and anything soiled by discharges immediately disinfected or destroyed. Cheap bath-houses. Easy running ambulances, or covered spring carts, with slung cots or stretchers. Dispensaries for diarrhoea and issuing disinfectants, gratis. Houses of refuge for inhabitants of stricken dwellings. Flannel belts to be worn always. Anything lowering health or strength to be avoided. Temperance and moderation in all things. Dead bodies to be at once placed in shells with absorbent substances containing dried disinfectants—sand, sawdust, cotton waste, &c., would do.

Ferran's inoculation was remarked on, and possibility of its ultimate success considered.

Houses to be cleaned; privies, ashpits, &c., to be ventilated; whitewash, mixed with disinfectants, to be used extensively; cellars to be dried and ventilated—"calorigen," or some such contrivance, useful. Nurses to be particularly clean, to lubricate with carbolic oil; only one-fourth of day on duty, three hours at a time; no food near sick to be used; particular watchfulness in morning and evening as regards nursing.

B. *Treatment, divided into four parts.*—1. *Malaise*—most important to watch for and treat; rest mind and body; light liquid food; stimulants, chiefly aromatic spirit of ammonium or carbonate of ammonium, tea, or small quantity of punch, not repeated; spirits very injurious. To these red pepper, tinct. capsic., or pepsin may be added to help to digest the poison, which a healthy stomach often does. Afterwards, small doses of quinine, calomel, opiates, and James's powder, hot bath, or nitro-muriatic foot-bath, to act on skin and liver, sinapisms to epigastrium, sweetened acid drinks of sulphuric acid (dilute), and pepsin—(Baillie) ox gall 30 grs., pepsin 15 grs., every half hour; (Corrigan's "shot") 20 mins. aromat. spirit of ammonium and 10 mins. tinct. opii, licked up from hand, often prevents.

2. *Diarrhoea*.—Liquor morphinæ, chlorodyne or tinct. opii, in combination with astringents and aromatics, at frequent intervals; suspend opium directly collapse threatens; solid opium never to be given except in first stage; hot water vessels; hot salt bags; cupping wet or dry, and



poulticing to loins, if kidneys are threatened; head and shoulders should be somewhat elevated, although discomfort produced; the patient to lie on the right side on a bed to be tilted to that side by blocks under legs, to relieve internal organs, promote circulation in liver, and help, by gravitation, to stop discharges, as also to let them drain away into disinfecting vessels, patient being laid on a water bed, or a waterproof sheet, with edges turned up. The following prescription to be used:—

R. Pepsinæ, . . . . gr. 36.  
 Acid. nitric. dilut., fl. dm., . . j.  
 Acid. sulphuric dilut., fl. dm., . . ij.  
 Acid. hydrochlor. dilut., fl. dm., . . iij.  
 Spirit. chloroformi., fl. dm., . . iij.  
 Aquæ destillat., . . . . ad fl 3 xij.

M.—Shake well. Half a wineglassful at once in an equal quantity of water. Toast water or cold beef tea, if much debility. And half above quantity every quarter or half hour afterwards, according to urgency of symptoms. To first dose 20 mins. of liq. morph., tinct. opii, or chlorodyne, and five mins. every alternate dose after, unless collapse threatens.

3. *Rice Water Stage and Collapse*.—Condition closely allied to tetanus, through contraction of muscles irritating terminal nerve fibres. Patient's head to be somewhat elevated by disinfecting pillows; hot saline injections into rectum. (Strahan—Minute doses of creasote, tinct. iodi., for cholera infantum, *why not useful here?*) (M'Farlane—1 gr. potas. permang., freely dissolved in 3 iv. distilled water; a teaspoonful every ten minues; hot oatmeal and water for drink). (Harkin—Blister over cervical portion of pneumogastric, right side alone often answers). Have plenty of boiling water ready, and use it and the solution to be described, as follows:—

R. —Sodii. chlorid., part. xij.; potass. chlorat., part. vi.; sodii phosphat., part. ii.; misce bene.

Two and a half ounces by weight, or, roughly, two and a half small tablespoonfuls, to be dissolved in twenty-four ounces of hot water, to which a little *alkali*, albumen of egg, or Liebig's meat extract, should be added.

Blankets doubled and wrung out of boiling water are to be sprinkled with the solution, and wrapped around patient above and below, leaving space at head, the whole to be covered by more blankets or waterproof sheets, and renewed as required, drawn under patient, who should not be allowed to move or sit up. Object of above to restore fluids, salts, sustenance, and warmth to system. Example of patient with dropsy contracting cholera. One disease cured the other:—Acid-pepsin mixture to be continued in small doses of carbolic acid. For thirst, give acid drink, cold water, or ice, if warm drinks not tolerated; carbonic acid draughts in small quantities, with mucilage and syrup (Loundes gave

with success liquid extract of raw meat at frequent intervals); *small* intravenous injections, often repeated; skim milk previously boiled and diluted with alkalisied water, warm; a blood serum of animals deprived of gases and corpuscles *in vacuo*, if human blood not obtainable (extemporised apparatus described), allowed to gravitate into veins, or hypodermic injections of same in water well distributed under skin by massage.

Drachm of ether in beef-tea to be injected into rectum to meet collapse. For cramps, *kneading* with oil or forcible flexion, or hypodermic injections of chloral hydrate, morphine, or water. Peritonitis not now so much dreaded as formerly, therefore it is suggested that non-irritating, aseptic fluids should be allowed to gravitate gradually as absorbed into abdominal cavity through needle, or canula, strapped outside transversely to abdominal walls to prevent injury by spasm of rectum. This would allow fluids to pass dried to intestines, spare drain on blood vessels and tissues "imitating" dropsy in case referred to.

Object of all the foregoing suggestions to enable patient to gain time and, by absorption, replace loss, and prevent disastrous effects through sudden abstraction of fluid.

4. *Reaction*.—Moderate this as much as possible. Absolute rest and strict attention to diet necessary to prevent irritation, inflammation, and repose. State of each individual the only guide for administration of medicine; but, as a general rule, mild diuretics and diaphoretics might be indicated by condition of viscera and blood.

DR. HENRY KENNEDY called attention to the use of saline injections into the veins, as adopted during the last epidemic in London. Experiments had been performed in presence of a number of students, and in some cases, after 80 ounces had been injected, the patient recovered. Having himself treated cholera during the epidemic of 1866, he discovered that when the patient fell into second collapse, the second sound of the heart entirely disappeared.

DR. H. C. TWEEDY said a French surgeon—M. Roullière—had recently published experiments on 55 cases of cholera in a very advanced stage of collapse, treated in the military hospital at Toulon. He had injected from  $1\frac{1}{2}$  to 2 grammes of serum in each case, and he spoke very unfavourably of the results, which Dr. Tweedy believed might have been due to the small quantity injected.

DEPUTY INSPECTOR-GENERAL BURKE then replied.

#### *A Case of True Relapse in Enteric Fever.*

DR. J. W. MOORE reported the case of Mrs. Mary B., aged twenty, a domestic servant, who was admitted to Cork-street Fever Hospital, under his care, on Saturday, January 24, 1885, on the ninth day of enteric fever. The area of splenic dulness was enlarged. There were several

typical rose-spots on the trunk, and also some taches bleuâtres, and there could be no doubt as to the diagnosis. The bowels were for some days free, but towards convalescence obstinate constipation was a prominent symptom. The pyrexia lasted for 27 days, then subsided by lysis. The patient in due time went to a convalescent home in the suburbs, where, after an apyrexial period lasting 24 days, febrile symptoms again set in. These ushered in another attack of true enteric fever, which ran its course in 24 days, and terminated favourably. There were rose-spots in this second fever, and moderate diarrhoea existed for four days. Towards the close constipation returned. Mrs. B. left hospital finally on April 15, nearly three months after her first admission, and has since enjoyed good health. Dr. Moore alluded to two theories as to the ætiology of relapse—(1) the re-contamination of the blood with the virus of the disease as a result of non-elimination, owing to constipation, and (2) a similar re-contamination of the blood in consequence of the commingling in the general current of the circulation after crisis of non-depurated blood, which had lain by in the enlarged and congested spleen, and so had escaped the purification of crisis.

DR. M'SWINEY said the case afforded a lesson to young practitioners, as it presented some obstacles to diagnosis.

DR. WILLIAM MOORE was under the impression that cases of relapse were more frequent than ten per cent.

DR. GORDON had no doubt in his own mind that the relapses and the second fever arose from the contamination of another set of glands, which probably had not been implicated at first. But the important and truly practical point in the case was this, that there was no complication which they ought to dread so much in typhoid fever as constipation, whether in the commencement or in the course of the disease.

DR. HENRY KENNEDY said that relapses in typhoid were more prevalent at some seasons than at others. He believed that very slight hæmorrhage indeed sufficed to modify very materially the fever.

DR. FINNY referred to the fact that at the present day constipation was the rule rather than the exception. The danger of constipation might be averted where the bowels could be moved by enemata. He laid great importance on the size of the spleen, and he did not consider a patient well until he saw the spleen reduced to its normal size. In reference to the secondary fever, Murchison, among other writers, suggested that rash was rare in relapse, and of a darker colour.

THE REGISTRAR-GENERAL (DR. GRIMSHAW) said Murchison did not consider there was *bond fide* relapse unless there were spots. Secondary fever could not be considered to be relapse of the original disease, which was extremely rare.

DR. JOHN WILLIAM MOORE replied.—True relapse was not so frequently met with as Dr. Kennedy seemed to think. The fevers that came on in

convalescence they were familiar with, but a true relapsing typhoid fever was very rare. Murchison's experience was 3 per cent. With regard to the duration of the enlargement of the spleen, he observed that the spleen subsided very considerably after the period of the first attack. He thought Dr. Grimshaw was going too far when he considered the re-appearance of rose-spots necessary. They knew that typhoid fever presented itself without a single rose-spot, and it was more philosophic and in the spirit of Stokes to consider that no one symptom was pathognomonic. In the first attack he suggested that the greatest mischief was low down in the ileum, and when relapse occurred they might infer that it was other glands high up in the ileum that were involved.

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#### URETHAN, THE NEW HYPNOTIC.

DR. VON JAKSCH, of Vienna, having been struck with the description given in the *Yearly Report on the Progress of Pharmaco-Therapeutics* by Dr. Kobert, of a substance named urethan ( $\text{NH}_2 \text{ CO}_2 \text{ C}_2 \text{ H}_5$ ), determined to make trial of its hypnotic effects, with the hope of being able to replace morphine, chloral, and paraldehyde, by a drug which would ensure sleep without causing any of the disagreeable or dangerous effects which are liable to follow their use. He first of all made experiments on animals, and found that no toxic action occurred in rabbits, even when the drug was administered in the proportion of .5 gramme for each kilogramme of the animal's weight. He therefore commenced his clinical observations by giving .25 gramme (3.85 grains) doses to patients. This, however, was insufficient to produce any distinct hypnotic action. He therefore raised the dose to .5 gramme (7.7 grains), and found this usually produced some hours' comfortable sleep. In a case of hemiplegia, with endocarditis and stenosis, and insufficiency of the mitral, where the patient had long complained of sleeplessness, but whose condition contra-indicated the employment of morphine and chloral, two doses at 7 and 8 30 p.m., given every night, produced a really good night's rest, without the least disagreeable effect. Again, in a case of a painful aortic aneurysm with insomnia, a dose given at 6 p.m. producing no effect, another was given at 11 p.m., which gave calm sleep till 3 a.m. The next night doses were given at 6 30, 9, and 11 o'clock, and a calm sleep was induced, lasting from 11 to 4. The following evening the first dose, at 6 30, produced a little sleep; another dose, at 8 45, gave sleep from 9 to 4 30, with a slight intermission; the third night two doses were given, resulting in six hours' sleep; the fourth night the patient slept without the drug. It had, however, to be again resorted to on the sixth and seventh nights. Altogether, Dr. von Jaksch employed urethan 110 times in twenty different cases, and is highly satisfied with the results he obtained.—*Brit. Med. Jour.*, Sept. 26, 1885.

## CLINICAL RECORDS.

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*A Case of Extra-uterine Fœtation, with Rupture of the Sac.* By JAMES TANDY BOLGER, L.K.Q.C.P.; L.R.C.S.I. Blaina, Monmouthshire.

Mrs. L., aged twenty-six, one child. The patient's illness commenced quite suddenly. About eight o'clock in the morning she felt severe pain in the hypogastrium. This caused extreme faintness, but she did not lose consciousness. When seen about two hours after this, she was still in a collapsed condition. Mrs. L. stated that she had last menstruated five weeks ago, and that before marriage she had experienced similar, though not so severe, pains during some of her periods. At next visit—about 3 p.m.—she had rallied considerably, the pain was not nearly so bad, the face had its usual colour, she was warm and had a fair pulse. There had been several attacks of dyspnœa, but during none of these had she been seen either by the gentleman with whom I saw the case or by myself; they were said to cause great distress. In addition she had had some vomiting, and from the first a strong impression that she would not recover. At about 8 p.m., when the next visit was made the patient was moribund, and died about 12 o'clock the same night. It was at the third visit that a diagnosis was made—viz., extra-uterine fœtation, with rupture of the sac. This was confirmed by a *post mortem* examination.

On opening the abdomen the pelvis was found filled with blood, mostly fluid, but with some quantity of clot. In clearing this out the ovum was removed on the sponge. The sac was found in the right tube, quite free from adhesion and of small size. The ovum appeared to be five weeks old.

Cases of extra-uterine pregnancy are fortunately rare, and many men of experience have never met with one. Sir Spencer Wells says:—"With the exception of one case recorded by Mr. Cooke, &c., and another case recorded by Mr. Doran, &c., I am not sure that I have ever seen any variety of extra-uterine fœtation either in my own practice or in consultation." It is rarer still for rupture to occur so early, and death to supervene so rapidly as in Mrs. L.'s case, and herein lies its chief interest. The difficulties in making a diagnosis were great, especially so to those who had never before to deal with the same conditions, for in an obscure case like this nothing can supply the place of experience, which alone will give confidence in arriving at a conclusion as to its

nature. When the true state of affairs was realised it was quite plain that it was too late to do anything.

When Mrs. L. was taken ill she had no suspicion that she was pregnant, but was under the impression that her menstrual period was coming on. She conceived this idea because the pain was like that which she had formerly suffered at such times, and also because it was only five weeks since she was last unwell. This was the first obstacle to a clear reading of the case. Then, again, at the second visit her appearance did not denote a critical condition. No vaginal examination was made, and I do not see what help it could have given, for with an ovum free in the abdominal cavity and a ruptured sac of five weeks' growth, there would have been no tumour capable of being felt, nor would the presence of fluid blood in the pelvis be readily made out. At the third visit, however, the signs of internal bleeding were unmistakable, and it appeared certain, from the symptoms already related, that the abdomen was the seat of the hæmorrhage, and probable that a ruptured uterine foetation was the cause of it.

At the present day I think no one would deny that the diagnosis of such a case being clear, or even probable, the proper thing to do is to open the abdomen, clear out the blood and excise the sac. In a more advanced pregnancy it is necessary to suture the sac to the edges of the abdominal wound and allow the placenta to separate. This, I believe, is what Mr. Lawson Tait does. To render an abdominal section justifiable the patient must have at least a prospect of recovery, but when the gravity of Mrs. L.'s case became apparent she was so far gone that no one, I think, would operate under such circumstances, especially when the unavoidable delay which must occur before the operation could be commenced be taken into account. The lesson of this case is, that if any good is to be done an early diagnosis is of paramount importance.

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#### ERYSIPELAS.

IN a communication read before the Académie de Médecine, MM. Trélat and Verneuil insist upon the importance of antiseptic treatment, and urge the isolation of cases occurring in hospitals. M. Cornil said that the difference in gravity between medical and surgical erysipelas is due to the fact that in the former the bacterium of erysipelas exists alone, while in the latter it is combined with the micro-organisms of pyæmia, and that in the latter class of cases it occurs in patients already weakened by previous sickness or traumatism.—*Phil. Med. Times*, Oct. 3, 1885.

# SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P., F. R. Met. Soc.

## VITAL STATISTICS

*Of the Eight Largest Towns in Ireland, for four Weeks ending Saturday,  
December 5, 1885.*

Towns	Population in 1885	Births Registered	DEATHS REGISTERED			DEATHS FROM SEVEN ZYMOTIC DISEASES							DEATH-RATE per 1,000		
			Total Number	Under 1 year	At 60 years and upwards	Smallpox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhoea	Deaths from Pneumonia	From all causes	From seven Zymotics
Dublin, -	353,082	749	743	128	192	-	2	14	2	27	18	10	96	27.5	2.7
Belfast, -	219,222	544	375	88	53	-	4	10	2	7	10	6	47	22.3	2.3
Cork, -	80,124	160	119	17	38	-	1	13	-	1	2	4	11	19.3	3.4
Limerick, -	38,562	77	60	10	20	-	-	-	-	2	1	-	8	20.2	1.0
Derry, -	29,162	52	67	8	20	-	-	-	-	4	-	1	5	29.9	2.3
Waterford, -	22,457	59	40	6	10	-	-	-	-	-	-	-	4	23.2	—
Galway, -	15,471	23	21	-	9	-	-	-	-	-	-	-	4	17.7	—
Newry, -	14,808	29	17	3	3	-	-	-	-	1	-	1	1	14.9	1.8

## Remarks.

In the eight selected towns included in the foregoing Table the highest death-rates are 29.9 per 1,000 of the population annually in Londonderry, 27.5 in Dublin, 23.2 in Waterford, and 22.3 in Belfast; the lowest rates are 14.9 in Newry, 17.7 in Galway, 19.3 in Cork, and 20.2 in Limerick. The rate of mortality from seven chief zymotics ranged from 3.4 per 1,000 per annum in Cork, 2.7 in Dublin, 2.3 in Belfast and Londonderry, 1.8 in Newry, and 1.0 in Limerick, to *nil* in Galway and Waterford.

The recorded deaths represent a rate per 1,000 of the population annually of 20.2 in twenty-eight large English towns (including London, in which the rate was 20.0), 23.7 in the sixteen chief towns of Ireland, 26.0 in Glasgow, and 20.6 in Edinburgh. With the advancing season, there is once more a marked increase (from 18.5 to 20.2) in the mortality in the English towns generally; in London it has risen rather more decidedly, namely, from 18.1 to 20.0 per 1,000 per annum. The mortality in the metropolis was each week 19.6, 19.9, 20.6, and 19.7 per 1,000 per annum respectively. The death-rate again rose decidedly in

Glasgow (from 23·1 to 26·0). In Edinburgh it rose also considerably (from 16·2 to 20·6). In the Irish towns the rate of mortality has likewise increased, from 20·8 to 23·7. If the deaths (numbering 13) of persons admitted into public institutions from localities outside the Dublin Registration District are deducted, the death-rate of that district becomes 27·0, while that of the portion of the district included within the municipal boundary is 29·9 per 1,000 per annum. In London the mortality from smallpox is still happily diminishing—the deaths were only 8 compared with 14, 29, 29, 63, 105, 193, 239, 179, 109, 197, and 229 in the eleven preceding periods of four weeks each. The weekly number of deaths from this disease since the beginning of the year have been 56, 42, 70, 58, 59, 60, 56, 44, 37, 27, 35, 23, 24, 39, 40, 49, 51, 48, 66, 75, 50, 55, 62, 39, 37, 28, 39, 13, 15, 18, 16, 14, 15, 5, 9, 6, 9, 8, 11, 7, 3, 0, 0, 9, 5, 3, 2, 2, and 1 respectively. The deaths from diarrhoeal diseases in the same city, which numbered 54, 51, 52, 78, 415, 1,163, 429, 166, and 73 in the nine preceding periods of four weeks each, were only 51.

Acute febrile zymotics were returned as the cause of death in 86 instances in the Dublin district, compared with a ten-years' average of 111·0 in the corresponding period and 64 in the previous four weeks. The 86 deaths included 2 from measles, 14 from scarlet fever, 18 from "fever," 27 from whooping-cough, 10 from diarrhoeal diseases, and 2 from diphtheria. There was no death from smallpox. The fatality of scarlet fever shows an unsatisfactory increase, the deaths being 6 more than in the previous four weeks. Of the 18 deaths referred to "fever," 2 only were ascribed to typhus, while 12 were attributed to enteric fever, and in 4 instances the exact nature of the fever was either not specified or was ill defined. The deaths from fever were equal in number to those registered (18) in the previous four weeks. Six children aged between one and five years succumbed to scarlet fever, but not one victim to this disease was under one year old. The deaths from whooping-cough rose from 12 in the previous four weeks to 27. Of the 27 victims of this disease, 15 were between one and five years of age, and 10 were infants of less than twelve months old. The 2 victims of measles were children aged between 1 and 5 years. One death from cerebro-spinal fever was registered in the first week of the period—in Grand Canal-street (No. 4 South City) District.

Measles caused but 4 deaths in Belfast, compared with 6, 6, 7, 38, 88, and 162 in the six preceding periods. Scarlet fever was fatal in 10 instances in Belfast, and in 13 cases in Cork.

Diarrhoeal diseases were credited with only 22 deaths in the eight towns, compared with 28, 56, 96, 68, 30, 32, 27, and 18 in the eight previous periods of four weeks. In London the weekly registered deaths from diarrhoeal diseases were 10, 13, 16, and 12 respectively.



In the Dublin Registration District 749 births and 743 deaths were registered, compared with 678 births and 638 deaths in the previous four weeks. The births were those of 402 boys and 347 girls. The deaths of infants under one year were 128 against 116 in the previous four weeks; those of persons aged 60 years and upwards were 192, compared with 154 in the previous period.

The deaths referred to pulmonary consumption in the eight towns were 176, compared with 146, 156, 197, 209, 213, 243, 277, 252, 270, 244, and 239 in the eleven preceding periods of four weeks each. In Dublin diseases of the respiratory organs are stated to have caused 155 deaths, against an average of 176·2 in the corresponding four weeks of the previous ten years, and compared with 142, 96, 98, 65, 117, 120, 177, 207, 246, 225, and 213 in the eleven preceding periods of four weeks each. The 155 deaths included 108 from bronchitis (average = 122·1) and 25 from pneumonia (average = 28·8). Of the 108 persons who succumbed to bronchitis, 22 were infants under twelve months, whereas no fewer than 32 had passed their sixtieth year.

On Saturday, December 5, 1885, there were under treatment in the principal Dublin hospitals no cases of smallpox, 1 case of measles, 36 cases of scarlet fever, 14 of typhus, 30 of enteric fever, and 5 of pneumonia.

The mean temperature of the four weeks was 44·4° in Dublin, 43·7° in Belfast, 46·8° at Roche's Point, Co. Cork, 40·2° at Edinburgh, 39·9 at Glasgow, and 42·8° at Greenwich. The minimal readings of the thermometer in the screen were 31·0° in Dublin, 34° at Belfast, 31° at Cork, 19·8° at Edinburgh, 15·5° at Glasgow, and 28·1° at Greenwich. The maximal temperatures were 58·2° in Dublin, 58° at Belfast, 56° at Cork, 57·6° at Edinburgh, 53·0° at Glasgow, and 58·2° at Greenwich.

The mean temperature was considerably below the average at all stations during the second week of the period, particularly at the Scottish stations (M.T. = 32·0° at Glasgow and 33·6° at Edinburgh); but in the other weeks the weather was mild, so much so indeed that the seasonal decline of temperature was but slightly marked in Ireland from the preceding to the present four-week period.

#### METEOROLOGY.

*Abstract of Observations made in the City of Dublin, Lat. 53° 20' N.,  
Long. 6° 15' W., for the Month of November, 1885.*

Mean Height of Barometer,	-	-	-	29·838 inches.
Maximal Height of Barometer (on 16th at 9 a.m.)	-	-	-	30·316 „
Minimal Height of Barometer (on 26th, at 11 p.m.),	-	-	-	28·799 „
Mean Dry-bulb Temperature,	-	-	-	45·6°.
Mean Wet-bulb Temperature,	-	-	-	43·9°.
Mean Dew-point Temperature,	-	-	-	42·0°.

Mean Elastic Force (Tension) of Aqueous Vapour, -	·270 inch.
Mean Humidity, - - - - -	87·6 per cent.
Highest Temperature in Shade (on 3rd), - - -	62·1°.
Lowest Temperature in Shade (on 15th), - - -	31·0°.
Lowest Temperature on Grass (Radiation) (on 15th),	24·8°.
Mean Amount of Cloud, - - - - -	66·5 per cent.
Rainfall (on 17 days), - - - - -	2·398 inches.
Greatest Daily Rainfall (on 26th), - - - -	·876 inch.
General Direction of Wind, - - - - -	S.E., E.

*Remarks.*

This was a mild, very dull month—south-easterly and easterly winds preponderating to a remarkable extent. As the prevailing winds were off the sea in Dublin, temperature was higher in that city than in most parts of the country. The rainfall was very small up to the 21st, only ·495 of an inch (not quite half an inch); but a stormy, rainy period followed, lasting nearly to the end of the month.

The mean height of the barometer was 29·838 inches, or 0·029 inch below the average value for November—namely, 29·867 inches. The mercury rose to 30·316 inches at 9 a.m. of the 16th, and sank to 28·799 inches at 11 p.m. of the 26th. The observed range of atmospheric pressure was, therefore, 1·517 inches—slightly more than an inch and a half. The mean temperature deduced from daily readings of the dry bulb thermometer at 9 a.m. and 9 p.m. was 45·6°, or 0·8° above the value for October, 1885; that calculated by Kaemtz's formula—viz.,  $\text{min.} + (\text{max.} - \text{min.} \times \cdot 41) = \text{Mean Temp.}$ —from the means of the daily maxima and minima was 45·1°, or 1·4° above the average mean temperature for November, calculated in the same way, in the twenty years, 1865–84, inclusive (43·7°). The arithmetical mean of the maximal and minimal readings was 45·9°. On the 3rd the thermometers in the screen rose to 62·1°—wind S.W.; on the 15th they fell to 31·0°—wind N. The minimum on the grass was 24·8° on the 15th. The past month was very mild, but not nearly so mild as November of 1881 (M. T. = 49·4°), which was by far the warmest November in the twenty years 1865–84. November was also warmer in 1866 (45·4°) and in 1874 (45·8°) than in the present year. It will be noted that there was actually an increase of temperature from October to November. The rainfall was 2·398 inches, distributed over 17 days. The average rainfall for November in the twenty years, 1865–84, inclusive, was 2·312 inches, and the average number of rainy days was 16·8. The rainfall, therefore, and the rainy days were both very slightly above the average.

There was hail on the 15th, and sleet fell on the 4th. A lunar corona was visible on the 21st. The atmosphere was foggy on the 11th, 12th, 15th, 21st, 22nd, and 23rd.

With the setting in of the month the weather underwent a decided change in one particular—namely, that the temperature rose above the average for the first time for many weeks. In other respects, the record of the first week was again one of unsettled, rainy weather. On Tuesday, the 3rd, the thermometer rose to  $62.1^{\circ}$  in the screen—a value which has been exceeded only twice in November since 1872; in that year the thermometer marked  $63.2^{\circ}$  on the 6th, and in 1881 it read  $62.3^{\circ}$  on the 14th.

Sunday, the 8th, was a fine autumnal day, but the weather was afterwards overcast and gloomy in the extreme until the evening of the 12th, when the sky cleared for a time and temperature fell, having been previously rather high and quite steady. During the dull period an anticyclone lay over Western Europe, its centre being found over Denmark and the Skager Rack, where the barometer exceeded 30.50 inches.

During the third week (15th to 21st inclusive) the weather in the British Islands was determined chiefly by an anticyclonic system—area of high barometer—while depressions passed eastward across Scandinavia, at the same time that an area of relatively low pressure lay permanently over France, the Bay of Biscay, and the Atlantic off the S.W. coasts of both England and Ireland. Some severe frosts were experienced in parts of Great Britain, but from the 17th gradients for E. to S.E. winds were established in the south, and temperature rose gradually, with increasing cloud and a softer, damper atmosphere. On the evening of Saturday, the 21st, a very beautiful lunar corona appeared in a filmy stratiform cloud. Scarcely any rain fell during the week.

Almost throughout the fourth week (22nd to 28th, inclusive) conditions were cyclonic in the British Islands, while an area of relatively high pressure held in Scandinavia and the Baltic. At first barometrical gradients were slight, but after Tuesday, the 24th, they deepened considerably, and both on Thursday night and Saturday morning very deep and extensive depressions passed northeastwards across Ireland and Scotland, causing strong S. to W. gales, high but unsteady temperature, and heavy falls of rain in most parts of our islands.

On the forenoon of the 26th there was a fresh S.E. gale in Ireland, and rain fell in torrents on the E. coast of Dublin and Wicklow. Early next morning there was a storm from S.W. veering to W. At night a splendid meteor shower was seen in most parts of Europe—the radiant point being in the constellation “Andromeda.” This display takes place at intervals of thirteen years, when the earth’s path intersects the orbit of Biela’s comet. After midnight temperature rose in Dublin to  $55.4^{\circ}$ .

Sunday, the 29th, was at first mild and fine, then rainy; but the 30th was a very fine, dry and bracing day.

## PERISCOPE.

### LANOLIN: A NEW BASIS FOR OINTMENTS.

UNDER the above title Dr. Oscar Liebreich delivered lately, before the Medical Society of Berlin, an interesting address, of which we shall give an abstract. The paper is published *in extenso* in the *Berlin. klin. Wochens.*, 47, 1885. Lanolin is a peculiar substance which possesses properties that do not appertain either to the glycerin fats or to the mineral oils. In 1868 Hartmann and Schulze showed that in the fat of sheep's wool the fatty acids were combined with cholesterin, forming peculiar fats; and later on Schulze proved the existence of isocholesterin in union with fatty acids. Hence we have, for the animal organism, a series of new fats. Berthelot in 1860 had prepared a number of cholesterin fats by heating together cholesterin and fatty acids. For the detection of a cholesterin fat Liebreich uses Liebermann's test, which depends upon a reaction of cholestol, a body allied to cholesterin. The test is sufficiently simple. A very small quantity of the fat to be investigated is dissolved in acetic anhydride (not to be confounded with anhydrous acetic acid). By the *cautious* addition of concentrated sulphuric acid a rose red colour is developed, which very quickly passes into a deep blue and green tint. Cholesterin fats, in which no trace of free cholesterin was contained, developed the cholestol reaction vividly, whereas neither any of the glycerin fats nor protagon, lecithin, spermaceti, nor beeswax, gave the reaction. Cholesterin fats are widely spread, and their presence was shown by the cholestol reaction in human skin and hair, in vernix caseosa, whalebone, tortoiseshell, horn-shavings, feathers of various birds, spines of hedgehog and porcupine, hoof of horse, &c. A most remarkable property of cholesterin fat is its power of taking up 100 per cent. of water. To this property, which was first exquisitely demonstrated in sheep's wool fat, Liebreich gives the name "lanolisation." A mixture of glycerin fats with cholesterin will not "lanolise." For plasters and ointments the vehicles utilised have hitherto been either the neutral glycerin fats or the residues of petroleum distillation (vaselin. paraffins). In the second edition of the "German Pharmacopœia," and in the new edition of the "British Pharmacopœia," a mixture of hard and soft paraffin is employed for some combinations. The fatty ointments readily decompose, especially upon the skin, and form irritating products, and, moreover, with pure fats the absorption of medicinal adjuncts is very imperfect. Vaseline, it is true, does not decompose, but it hinders the penetration of drugs into the skin, so that even highly poisonous substances rubbed up

with vaselin, and brought into contact with the skin, cause neither local nor general toxic phenomena. The mixed "*Unguentum Paraffini*" must be looked on as a still less suitable substance than vaselin, because, even with the most careful manipulation, gradually the individual constituents separate from each other, and nothing can be better established than that ointments, *e.g.*, of iodide of potassium, white precipitate, &c., are perfectly inert as paraffin ointments, and Liebreich always recommends the old plan of dispensing in fatty vehicles. With cholesterin fats, on the contrary, it is surprising how readily they penetrate into the skin. The combination of neutral cholesterin fat with water Liebreich terms "*lanolin*." Over 100 per cent. of water can be worked up with the fat, and the mixture forms a thick emulsion with soaps and alkalis. The fat, besides, readily absorbs glycerin, and this mixture can again be mixed with any other fat with facility, and pleasant creamy ointments are thus procurable. Lanolin has a slight odour. As a practical point it may be mentioned, in contrast to fatty or vaselin ointments, that a morsel of a 5 per cent. lanolin-carbolic-ointment, the size of a pea, rubbed into the hand, causes, after one or two minutes, a numb feeling without any caustic action. Inunction of a lanolin-sublimate ointment (1 in 1,000 sublimate) brings out, after a few minutes, the characteristic metallic taste. Liebreich recommends that 5-10 per cent of fat or glycerin should be added to the lanolin.

W. G. S.

#### VACCINATION SHIELDS.

THE following note has been issued from the office of the National Vaccine Establishment, Whitehall, London, S.W. :—"The Medical Officer of the Local Government Board for England occasionally hears of cases of erysipelas following vaccination, and traceable to the use of old and dirty '*Vaccination Shields*.' If in any case, as where a dress is worn dyed with a possibly irritative dye, a vaccinator thinks some means of 'protection' to a vaccinated arm to be desirable, he had best define the material and the manner of application of such appliance as he judges to be wanted in the particular case; and it appears to the Medical Officer important that every such appliance should be of a kind to be destroyed and replaced whenever it becomes soiled; and, particularly, that it should not be of a kind likely to be kept for subsequent use. The Medical Officer would, therefore, urge on vaccinators to discourage the use of the so-called '*Vaccination Shields*.'"

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# THE DUBLIN JOURNAL

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## MEDICAL SCIENCE.

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FEBRUARY 1, 1886.

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### PART I.

### ORIGINAL COMMUNICATIONS.

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ART. IV.—*Axis-Traction in Instrumental Delivery, with Description of a New and Simple Axis-Traction Forceps.*<sup>a</sup> By WILLIAM C. NEVILLE, M.A., M.D., and Master in Obstetric Science, Univ. Dubl. ; M.K.Q.C.P.I.; Fellow, Honorary Secretary of the Obstetrical Section, and Member of the General Council, Academy of Medicine in Ireland; Physician to the Pitt-street Institution for Diseases of Women and Children, Dublin, &c.

THAT the principle of axis-traction in delivery by the forceps has hitherto been so little appreciated, except by specialists, is, I think, chiefly due to the comparative cost and complexity of the special instruments required rather than to any fault in the principle itself. Some weight must also be attached to the hesitancy felt by most men to abandon old grooves of thought or action. And this feeling is all the more natural in the present case, where it is sought to replace a simple and time-honoured instrument by one which *primâ facie* is much less easy to manipulate, and which (in the beginning at least) claimed acceptance upon the grounds of theory rather than of long and well-tried utility. Whatever its defects, the old double-curved forceps had served as well, preserving through all its modifications the same general construction that it had when first adopted with a curious simultaneousness by Pugh, Levret, and Smellie. But the more useful and the more essential an instrument is, so much the more ought we strive to eliminate its

<sup>a</sup> Read in the Obstetrical Section of the Academy of Medicine in Ireland, Nov. 27, 1885.

defects and to add to its powers. Nor need we be discouraged by the number of workers or the multitude of their failures.

Certain defects of the double-curved forceps have long been recognised, and quite a number of more or less successful attempts had been made to neutralise them before M. Tarnier commenced his work upon the improvement of the instrument. Those, however, who have studied his extremely able essays upon this subject cannot withhold from him the credit not only of having exposed with freshness and clearness the defects of the forceps as commonly used, but also of having been the first to construct one which answered to the true demands of theory. But Tarnier's forceps, even in its latest and most convenient form, is somewhat complex and expensive. Its general plan has been adhered to, and its construction simplified by Professor A. R. Simpson, whose modification has been endorsed by Drs. Barnes and Playfair. This is the axis-traction forceps chiefly used in this country, and the only one with which I have had personal experience. Modifications of Tarnier's forceps have been devised by Professors Hegar (Freiburg), and Lusk (New York), and others. Before dealing with the various questions concerning axis-traction forceps I shall state, as clearly as I can, the mechanical defects of double-curved instruments, such as that of the late Sir J. Simpson, or of Dr. Barnes. Two principal defects may be specially considered :—

1. *It is impossible, in using them, to exert the whole of the traction force in the curve of the pelvic axis—i.e., along the curve of least resistance to delivery.* A certain excess of traction force must be exerted, some of the force being wasted in a vicious pressure upon resisting maternal parts. This fault is the mechanical result of the conditions under which traction is made with any ordinary double-curved forceps, whatever the position of the head at the time of its application. Let us take, for example, such a forceps applied to a head engaged in the pelvic brim. The mechanical conditions will be understood by referring to the accompanying woodcut. The forceps being correctly applied in the sides of the pelvis, the axis of its pelvic curve may be assumed as corresponding—quite sufficiently so for practical purposes—with the axis of the upper part of the pelvic canal. Traction being now made on the head, the traction force must, on mechanical principles, take effect in some such direction as is indicated by the line  $TF$ —i.e., the axis of actual traction must fall more or less in front of the axis of desirable traction,  $TA$ . But descent of the head must take place along the

curve of the pelvic axis, ccc, or from its present position in the direction of the axis of the pelvic inlet,  $\tau A'A$ . Applying now the principle of the "parallelogram of forces," we may take  $\tau F' =$  to the actual traction force used, and resolve it into the two component forces,  $\tau A'$  and  $\tau P'$ —the former acting in the axis of the inlet, and the latter at right angles to it along the plane of the inlet. The component force  $\tau P'$  represents a vicious pressure against the pubes, which could be avoided if we were able to exert the primary traction force altogether profitably in the direction  $\tau A$ . If we know the angle  $A'\tau F'$ , and the traction force  $\tau F'$ , we can evidently determine the actual amounts of the two forces  $\tau F'$  and  $\tau P'$ . Tarnier, to whom this demonstration is due, calculated that a traction force of 40 kilogrammes applied to the head at the brim would be equivalent to a force of 30 kilogrammes acting in the axis of the inlet + one of 26 kilogrammes acting in the direction  $\tau P$ . In

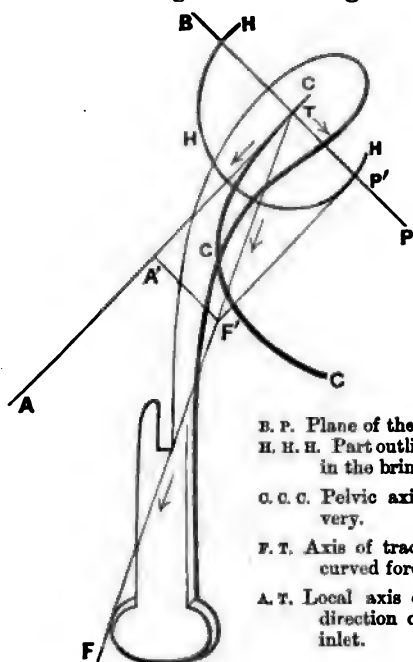


Fig. 1.

- B. P. Plane of the brim.
- H. H. H. Part outline of head engaged in the brim.
- C. C. C. Pelvic axis, or axis of delivery.
- F. T. Axis of traction with double-curved forceps upon point T.
- A. T. Local axis of traction in the direction of the axis of the inlet.

practice so great a waste of force can, it is true, seldom occur, at least when the forceps is in skilled hands. No expert would employ traction simply in a line continuous with that of the handles. But whatever corrective device he adopts can only give a haphazard correctness to the traction.

We see, therefore, that the force being misapplied, some of it is wasted in what must needs be a more or less injurious pressure upon maternal structures. Nor is the injury to the mother only. It follows also as a corollary from the preceding argument, that *the ordinary forceps is much more apt to slip off and bruise the fetal head than such a one as would pull constantly in the curve of the pelvic axis*. For with the former the traction is applied slantwise in a direction downwards and somewhat forwards against an anterior (pubic) resistance. Hence, when this resistance is great, there is a likelihood that the forceps may slip forwards off the head; or, what amounts to the same thing, that the head may be forced backwards from between the blades of the forceps. This danger is obviated by an instrument which would draw upon the contained head directly in the proper axis of delivery.

2. The second defect of the ordinary forceps consists in the *restraint which it necessarily puts upon the head while traction is being made*. It may enforce movement in a certain direction, but in doing so it must also greatly hamper the movements by which the head tends to accommodate itself to the varying resistances which oppose its passage through the pelvis. Delivery is thus effected at the cost of extra force employed and extra pressure brought to bear upon maternal structures. It has been argued that even during traction the sensitive hand takes note of and even helps such movements of accommodation. I very much doubt whether, during difficult forceps deliveries, involving the expenditure of much muscular effort, such a degree of sensitiveness can be claimed even for an unusually well-educated hand; nor can we forget that the forceps is not always in the hands of an expert. But rotation most commonly occurs, nevertheless, despite the form of the traction which opposes it, and the accompanying rotation of the forceps handles informs the operator—not before but after—the commencement of the movement. It is quite impossible to contend seriously that the operator can know from his sensations when and to what extent exactly rotation should occur. The most that can fairly be expected of him is that, when the head is rotating, care should be had not to hamper the spontaneous movement by too rigid a grip of the forceps handles. But the head is generally best left to pick its own way through the pelvis—hampered as little as possible by the forceps, which should provide motive power only.

What is wanted is a forceps so constructed as to admit of the whole force employed being exerted in the true axis of delivery, or

approximately so, and which would, at the same time, leave the spontaneous movements of the head uninterfered with.

Many of the older obstetricians recognised the desirability of making the axis of traction coincide, if possible, with the axis of delivery, and felt the failure of the classical forceps in this respect. Attempts were then naturally made to remove or minimise this defect. One of the most commonly practised of these consisted in a manœuvre which still usefully survives. To the tractile force as usually employed is added another force, pulling almost directly backwards in the vicinity of and just above the lock. While the chief operator pulls on the handles in the usual way, an assistant pulls almost straightly backwards by means of a napkin looped round or through the shanks of the forceps. The effect of these two forces simultaneously applied is to neutralise the vicious pressure towards the pubes which would result from the first alone. Their resultant is a single force acting more or less nearly in the proper axis. This manœuvre, to be properly tried, requires the presence of an assistant, and is, no doubt, of much help in difficult cases, where the head is at the brim, and a proper axis traction forceps is not at hand. In such cases it enables the operator to try traction in some new axis at least, though not—save as a matter of chance—in the true pelvic axis. It is obviously too haphazard a method to rely on for finding the letter with anything like certainty.

Towards the beginning of this century, Stein recommended the employment of traction by means of a fillet passed through the fenestræ as a corrective and supplement to traction by the forceps handles. Instead of the fillet, steel tractors, loosely fitted into the fenestræ, have of late been recommended for a like purpose. In 1844 Hermann, of Berne, devised a forceps, the special feature of which, in addition to a peculiarly accentuated pelvic curve, consisted in a T-shaped tractor which could be fitted to the shanks close above the lock. This tractor could be fixed at will, either on the anterior or posterior surfaces of the shanks, with the object of either pulling or pushing them backwards while traction was being made on the handles. Hermann's tractor was, therefore, really only a special appliance for carrying out the manœuvre already described. Hubert, of Louvain (1860), hit upon the plan of making a forceps with handles bent backwards at right angles about their middle. Subsequently Hubert did away with the bent handles, substituting in their stead a steel bar which jutted backwards from between the handles immediately below the lock. The principle of these forceps—

that traction acts in a direct line from the point at which it is applied to that on which it acts—was exactly the same as that subsequently utilised (1868) in Aveling's sigmoid forceps. Indeed, Hubert expressly alluded to the advantages of a forceps having the general form of an Italian S.

These few typical examples suffice to show how, before Tarnier's time, one defect of the classical forceps was commonly recognised. The common aim of these and other attempts at forceps reconstruction was to obtain an instrument by means of which the axis of traction could be brought, approximately at least, into coincidence with that of the pelvis. In this aim they only partly succeeded. Such an example as Aveling's forceps will serve to demonstrate the cause of failure. If, with this forceps, traction be exerted from the very extremity of the handles, the line of the pull would, doubtless, be nearly correct. But, in practice, such traction is impossible, or almost so, with this instrument; the handles must be grasped over so large a portion of their extent as almost to obliterate their compensatory curve, and make the line of traction differ but little from what it is with common forceps. The recurved traction-bar or sharply reflexed handles of Hubert's forceps certainly gave a less delusive advantage. But none of these forceps gave any certainty, by their construction, that the axis of traction would really coincide with the axis of the curved blades—*i.e.*, practically with the pelvic axis itself; none of them eliminated the varying effects of side-strains ("twists"), without which, in such cases, it is practically impossible to exert powerful efforts by traction applied over a large surface; and none of them even touched the important question of leaving the head unhampered, so far as its accommodation movements are concerned, during its enforced descent through the pelvis.

It is needless here to consider the various stages through which Tarnier's forceps went before reaching that in which it is now used by him and his adherents—suffice it to describe the latest and best model constructed in 1879. This consists essentially of an ordinary double-curved forceps (French type). Along the convexity of the lower part of the blades, just below the level of the fenestrae, two steel rods (T R) are attached by pivot joints (J 1). These are the "traction-rods" which lie parallel with the shanks, and terminate a short way above the lock of the forceps. When not in use they can be kept fastened to the shanks. When the forceps has been applied and locked these rods are loosened and fitted into a peculiar

rigid socket, or lock-joint (L), which is continued into a sharply recurved steel bar (T B) attached below, by what amounts to a universal joint (J 2 and J 3), to a transverse "traction-handle." The whole instrument consists then of two parts with distinct uses—one, the ordinary forceps, which serves the purposes of prehension and application; the other, a specially constructed apparatus for traction. And, inasmuch as traction is made solely by the latter, a "fixation-screw" (F S) is necessary to keep the blades in firm apposition to the head while traction is being made.

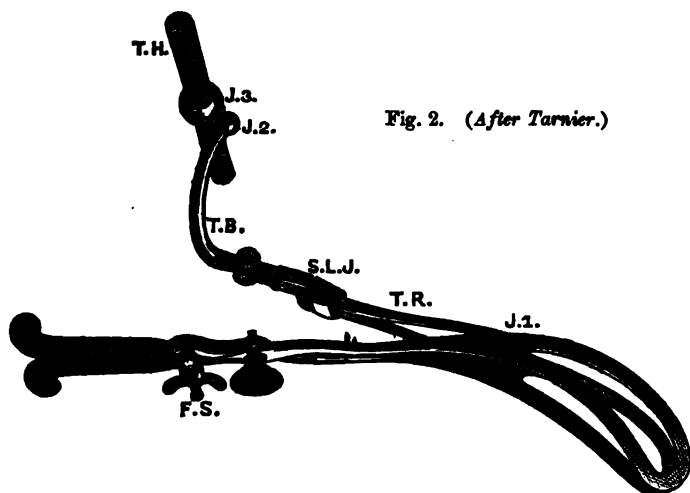


Fig. 2. (After Tarnier.)

- T. R. Traction-rod.
- T. B. Traction-bar.
- T. H. Traction-handle.
- J. 1. 1st joint.
- J. 2. 2nd "
- J. 3. 3rd "
- F. S. Fixation screw.
- S. L. J. Sliding lock-joint, uniting traction-rods to traction-bar.

Let us now examine the powers of this mechanical contrivance. Tarnier claims:—

1st. That by it traction is made in the direction of the pelvic axis.

2nd. That the force acts as nearly as possible to the centre of the head.

3rd. That during delivery the head preserves its mobility unimpaired.



4th. That with it the operator has a constant guide as to the movements of the head, and the direction in which he is to apply his traction.

The construction of the forceps is such that so long as the traction-rods are kept parallel to the shanks of the forceps, the axis of traction must correspond with the axis of the blades—*i.e.*, approximately with the axis of that part of the pelvis in which the head is situated. Now, as the head descends into the pelvis, the application-handles of the forceps turn spontaneously forward under the pubic arch, being allowed to do so by pivot-joint J 1. All the operator has to do, therefore, is to follow the handles forwards, keeping the traction-rods parallel to the shanks. The joints J 2 and J 3 allow the head to rotate freely, even during traction, and the movement of rotation is at once indicated by the partial rotation of the application-handles. Hence, as shown by Tarnier, these handles not only indicate the direction in which traction is to be exerted, but also the movements undergone by the head during delivery. Supposing the forceps to have been properly applied—the axis of its curved blades coinciding with that of the pelvis—then it results that traction must be applied directly, and as nearly as possible to the centre of the head. In other words, traction is made directly upon the head in the line in which it is expected to descend, and not at an angle to that line, as has been proved to be the case with the ordinary forceps. Hence the less danger of the blades slipping or injuring the head during delivery.

Another advantage of Tarnier's model has not received the attention which it merits. This is, that the force is applied from a fixed point which cannot be altered or in any way affected by the operator; traction being made upon the cross-handles, and the force is transmitted from this point directly to the blades, all side-strains being avoided by the universal jointing of the traction apparatus. Thus we know exactly the kind and direction of the force we employ—the latter of course being variable according to the angle which the traction-rods make with the shanks of the forceps. The nearness of the perinaeum limits, however, our powers of varying the direction of traction. The mechanism of this forceps must be considered, therefore, as justifying its inventor's claims for it. We find in it a great and scientific advance upon all the older instruments. Its defects are in the matter of constructive details and not of principle. Keenly criticised as it has been, the number of its modifications already designed by

leading obstetricians in all countries shows that its principles at least can no longer be successfully disputed.

Criticism, however, has not been wanting. In this case it has been of two kinds—dealing, firstly, with the principles; and, secondly, with the particular construction of the forceps.

Among critics hostile upon the question of principle, the eminence of M. Pajot entitles his observations to chief consideration. He objects to this forceps because:—

1st. It is an instrument “deprived of all action as a lever, an action of greater efficacy than that of simple traction.” The latter dogma cannot be argued here—suffice it to say that its correctness is much more than doubtful.\* The former statement is misleading, since there is nothing to prevent a trial—should such appear necessary or desirable—of lever-action (through the application-handles) at the same time that traction is exerted through the special traction apparatus. I have employed both of these forces, with apparent advantage, while using Simpson’s modification. In difficult cases it is well, from time to time, to test the hold the blades have of the head, and this can be done by using the left hand for traction, while the right one employs a leverage action through the application-handles.

2nd. The proper direction for traction cannot be ascertained “when the head remains mobile above the brim, and does not yield to energetic tractions.” Admitting this to be in a manner true, how is the ordinary forceps any better in such a case? Is not the correct axis of traction, under these circumstances, much more likely to be attained with Tarnier’s than with the ordinary forceps?

3rd. The same answer may be made to the criticism that the application-handles cannot indicate the direction for traction when the head becomes impacted.

4th. M. Pajot admits that with this forceps traction may be made almost in the axis of the pelvis, but regards this advantage as superfluous, “the pelvis itself being the true redressor of faults in traction.” Few, I think, will agree with an argument which, if it means anything, means that it is superfluous to economise force and to save maternal and foetal structures from a vicious pressure, because, despite all the waste and bruising which results from a misapplication of force, the pelvis may cheerfully be entrusted with the rectification of our mistakes.

\* *Vide* Galabin, *Obstetrical Journal*, Nov., 1876.

The fixation-screw, a necessary adjunct to Tarnier's forceps, has been objected to as likely to cause a dangerous because continuous pressure upon the foetal head. Now, this is a fixation and not a compression screw—attached close to the lock, where it has least power of compression. Nor should it be forgotten that the screw can be readily relaxed during the intervals between tractions, while, with an ordinary forceps properly applied and locked, extreme compression is made all but impossible by the breadth and spring of the blades. But more important than any presumptions on such a subject is the fact that an immense clinical experience with Tarnier's forceps and its modifications has failed to sustain the objection made to the screw.

A more plausible objection than any of the above is that during delivery by Tarnier's forceps we lose to some extent that sentient knowledge of the grip and progress of the head which we get from a broad grasp upon the rigid handles of the classical forceps. It has been stated that with the axis-traction forceps the blades may slip slowly off the head without the operator being aware that they are doing so, because of their jointed traction-apparatus. But, as a matter of experience, borne witness to by many who have used both forms of forceps largely, this slipping occurs quite rarely with the axis-traction forceps when once properly applied. And even if slipping does occur, it is likely to be more gradual and in a safer direction during the use of this than of the ordinary kind of forceps. Under similar conditions the traction-force is less, and the jointing of the traction-apparatus affords an additional safeguard against sudden violence. The indicating handles show the progress of the head at least as well as it can be felt, even by the most educated hand, during difficult delivery, involving exhausting tractions with the common forceps. And, if the operator be in doubt, he can judge of the grip by feeling the head at intervals through the application handles, even supplementing tractile with lever movements, as before explained.

The foregoing are the chief objections which have been made to Tarnier's forceps, and none of them seem to bear close examination. Besides, in such controversies, to score a point is not to win a victory, though critics commonly assume that it is so. The faults of this forceps—its unwieldiness, complexity, and the undeniable clumsiness of some of its parts—notably, of the junction between the traction-rods and the traction-bar, are faults of detail and not of principle, or even of general mechanism. The idea is here

found—in the rough it may be—but such as it is, a fertile idea, which in practical application only can be improved on.

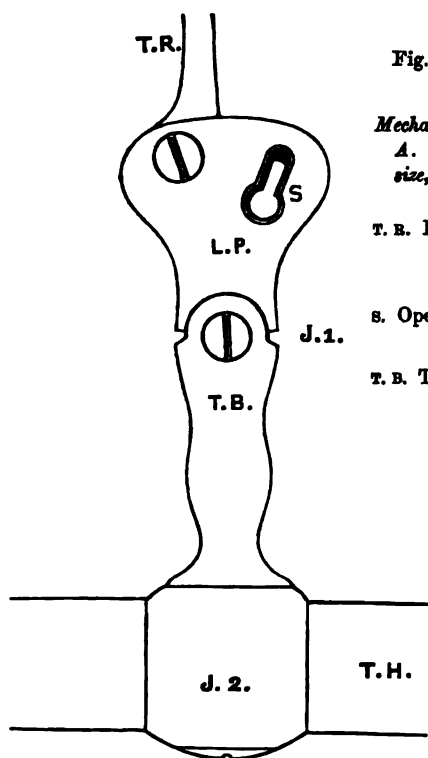


Fig. 3. (After A. R. Simpson.)

*Mechanism of Traction-Apparatus in  
A. R. Simpson's Forceps. (Full  
size, after A. R. S.)*

T. R. Left traction-rod attached by  
screw to flat "locking-plate,"  
L. P.

S. Open slot for reception of button  
end of right traction-rod.

T. B. Traction-bar, attached to lock-  
ing-plate by screw-joint, J. 1,  
which permits of lateral motion,  
and to traction-handle,  
T. H., by rotatory-joint, J. 2.

To Professor A. R. Simpson we are chiefly indebted in this country for having been the first to advocate the principles of the new forceps. He devised, besides, a simple and manageable modification of Tarnier's forceps, which has been largely used. The main points that are noteworthy in this modification are—the traction apparatus is here grafted on a somewhat modified and lengthened Simpson's forceps, which has the advantage of being less cumbrous than the French model used by Tarnier. The entire traction-apparatus is permanently fixed to the forceps, so that there are no separate parts of the instrument. The traction-rods, as in Tarnier's forceps, are attached beneath the fenestræ by pivot-joints; but that which belongs to the left (*i.e.*, the lower) blade is screwed on to a flattened "locking-plate," which is united by a

joint that permits of lateral movement to the "traction-bar." This latter finally passes at a right angle through a rounded opening in the transverse "traction-handle," this mode of attachment allowing of rotatory movement of the whole forceps. The blades having been applied to the head the free traction-rod is attached by a button on its extremity into an open slot in the locking-plate. The accompanying illustration (Fig. 3), after one by A. R. Simpson, shows diagrammatically the nature of these arrangements. The compensatory curve in this forceps is placed in the traction-rods, which lie alongside the shanks until close to the lock, from which they curve sharply backwards. In Tarnier's forceps, we have seen that the traction-rods terminate above the lock, the compensatory curve being introduced into the comparatively lengthy traction-bar. In Simpson's, as in Tarnier's forceps, traction in the axis of the curved blade is insured by maintaining parallelism between the traction-rods and forceps shanks.

Over Tarnier's model Simpson's has the advantage of greater lightness and simplicity. The greater length of the traction-rods in the latter allows in every case of the necessary attachments being completed without hampering by the closeness of maternal parts. The locking arrangement is also simpler and more manageable. But, as often turned out by instrument makers, the locking is not sufficiently rigid or exact, so that the strain falls unequally upon the two rods during traction, while the right one is constantly falling out of its slot. To one who is not experienced in forceps operations, or who has not read Professor Simpson's directions, one of the chief objections to his forceps must be the feeling of entanglement during the introduction of blades permanently encumbered with the entire traction-apparatus. Not that there is any real difficulty, but that there is an apparent one, which is very apt to be regarded by many as real, and inclines them to leave advantages which they do not clearly understand in the hands of specialists. Professor Simpson specially advocates this permanent attachment on the grounds that axis-traction is *always* desirable in forceps cases, and that any separate portions of the necessary instrument may be forgotten or mislaid. If the separable part was a small one there would be much force in this argument. As it really is, however, no believer in axis-traction would be more likely to forget his traction-bar handle than he would one blade of his forceps or a catheter when going to a labour case. Besides, in designing a very useful instrument neatness, simplicity, and portability are not to be

sacrificed to the detriment of many for the possible benefit of a few. So far as regards this feature in the forceps I certainly prefer the simple device of Professor Lusk, in whose modification the traction-rods are simply fitted into two open slots of a locking-plate, connected by a jointed bar with a traction-handle, these parts being separate from the rest of the instrument, as in Tarnier's.

[To be continued.]

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ART. V.—*Case of Abscess of the Spleen, occurring in Enteric Fever.*\* By ROBERT S. ARCHER, M.B., M.Ch., Univ. Dubl.; Physician, Netherfield Fever Hospital, Liverpool.

ON assuming duty at Netherfield Fever Hospital towards the end of last month, I found a boy, aged fourteen years, suffering from what appeared to me to be an intercurrent relapse or recrudescence of enteric fever. He had been admitted to hospital on Oct. 5th, 1885, under the care of my colleague, Dr. Dunbar, with a history of illness of two days' duration, which commenced with anorexia, vomiting, and rigors. His throat is reported to have been sore, and the tonsils superficially ulcerated. He, as far as I could ascertain, presented none of the common signs or symptoms of enteric fever—in fact, I believe he was sent to hospital as suffering from scarlatina, which was doubtless owing to the slight throat symptoms, and a doubtful evanescent red rash. The temperature, which had been febrile since his admission became normal on Oct. 18th, or about the sixteenth day of apparent illness. It remained normal till Oct. 21st, when it again became febrile, and remained so till the patient's death on Nov. 1st.

Oct. 24th was marked by slight pain along the course of the lower ribs on the left side, which was thought might have been due to pericarditis or pleuritis; however, careful examination failed to detect the existence of either of these conditions. On Oct. 26th the left hypochondriac pain became more marked, and on the evening of the following day there was slight general abdominal tenderness. The next twenty-four hours was marked by vomiting, intensification of the abdominal tenderness, and increased tympanites, especially manifest along the course of the transverse colon. All these signs became more painfully apparent during the next three days, and there was superadded a certain amount of stupor and low muttering delirium. The patient died early on

\* Read at a meeting of the Liverpool Medical Institution, Nov. 10, 1885.

the morning of Nov. 1st, apparently from peritonitis, due, as was thought, solely to perforation of the intestine.

**CASE.**—*Post-mortem* examination made ten hours after death. Fairly marked rigor mortis; considerable emaciation. Abdomen generally and equably distended. Distinct general purulent peritonitis. The omentum and coils of the intestines were loosely held together by layers of yellow puriform lymph. A large quantity of yellow fluid, which had evidently escaped from the intestines, occupied the depending portions of the abdominal and pelvic cavities. The intestines contained some yellow semi-solid fæces. There was a perforation situated about 1½ feet from the ileo-cæcal valve, occupying the base of a cicatrising ulcer. Several of Peyer's patches were observed to be ulcerated, quite free from sloughs, and evidently in process of cicatrisation. The long axis of the ulcers was usually transverse to the course of the intestine, owing, I suppose, to the fact that cicatrisation had commenced from each end, and not laterally. There was a mass of almost entirely cicatrised ulceration in the neighbourhood of the ileo-cæcal valve. The liver was enlarged and pale. The spleen was enlarged and generally soft and congested; its convex surface was covered by a more or less uniform layer of yellow puriform lymph; in its upper part, and under the surface of its external aspect, was an abscess which would contain a large-sized walnut. This collection of pus had ruptured into the peritoneal cavity. The lungs were found to be hypostatically congested, but otherwise healthy. The pericardium contained a considerable quantity of clear straw-coloured fluid. The heart was healthy.

**Remarks.**—Abscess of the spleen—a rare affection under all circumstances—would appear to be, contrary to what might be expected when we have regard to the fact that this organ is always affected to a greater or less degree in enteric fever, of very infrequent occurrence in this disease. Murchison's vast experience gives only two cases, both of which died, in what appeared to be a relapse, from general peritonitis, and he mentions five others from different authors. A considerable number of necropsies on the bodies of patients dying of enteric fever at Netherfield Fever Hospital during the last five years supplies only this single example of splenic abscess.\* As regards diagnosis, it may be mentioned that in this case the actual condition of affairs, as far as the spleen was concerned, was not determined clinically. But if in another case

\* In November, 1883, Dr. Coates showed at the Calcutta Medical Society a boy, aged four, with enlargement of the spleen, the result of malarious fever. An incision was made into an abscess and about 10 oz. of pus discharged.—*Brit. Med. Journ.*, July 19, 1884, p. 143.

there were presented for our observation, tenderness in the region of the spleen, pain under the left ribs, and a "creeping" peritonitis gradually extending from this as a centre over the abdomen, the existence of splenic abscess would be suggested to the mind; and further, that the abscess had burst and set up general inflammation of the serous membrane would be quite a reasonable conclusion to arrive at.

Although the boy was reported to have taken ill only two days before admission to hospital, judging from the condition of the ulcers I am strongly inclined to believe he must have been going about with the fever on him for at least a week before the apparent onset of the disease.

The reappearance of febrile symptoms after an apyrexial state of three days cannot be regarded as a relapse in the true sense of the word, but must be looked upon rather as symptomatic of the splenic mischief, just as in this disease, as well as in any other of the specific fevers, a continuance of the fever beyond the usual time, or a reappearance of fever accompanied by manifest local lesions, must be regarded as purely symptomatic.

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ART. VI.—*On Displacements of the Ovaries.*\* By THOMAS MORE MADDEN, President Obstetric Section Academy of Medicine in Ireland; Obstetric Physician Mater Misericordiæ Hospital; Physician, Hospital for Sick Children, Dublin; Consulting Obstetrician National Lying-in Hospital; Formerly Examiner in Obstetric Medicine and Gynæcology, Queen's University; and Vice-President British Gynæcological Society, &c.

DISPLACEMENTS of the ovaries, apart from those caused by ovarian disease, until recently were generally ignored by gynæcologists, and still attract less attention than their pathological importance demands. From clinical experience I have been long convinced that ovarian herniæ are of more common occurrence than is supposed even by Dr. Barnes, by whom this subject has been ably discussed in the *American Journal of Obstetrics*. In everyday practice a certain proportion of our gynæcological patients complain *inter alia* of some degree of dull sickening left-side pain, the situation of which, though not always clearly defined, is usually referable to the inguinal region. If further investigation

\* Read in the Obstetrical Section of the Academy of Medicine in Ireland, Friday, Jan. 8, 1886.



be instituted in such cases we may, in not a few instances, be able to trace this pain to ovarian displacement, which, however, is too commonly passed over without recognition, owing to the greater prominence of other symptoms.

Ovarian herniæ may be found in the inguinal region, and may be either direct or oblique. In the former the tumour appears in the groin above Poupart's ligament; in the latter it follows the course of the canal downwards and forwards, and makes its way into the labium. Occasionally the displacement is observed in the femoral region, immediately below Poupart's ligament, and to the inner side of the femoral vessels. But still more frequently the ovary is displaced downwards into Douglas's space, and this prolapse may, for all practical purposes, be here considered as a form of ovarian hernia. In these cases the left ovary, as from its anatomical position might be anticipated, is that usually prolapsed into the recto-vaginal fossa, where, on examination, it may be discovered as a small, oblong, elastic and highly sensitive tumour, bulging into the post-cervical vaginal *cul-de-sac*.

Although in some instances congenital, these herniæ most commonly occur in patients whose abdominal parietes have been relaxed, and viscera compressed, by repeated gestation. They may also be induced by similar immediate causes as other herniæ—such as the violent muscular efforts of the second stage of labour, lifting a heavy child, straining at stool, &c. But in the most frequent of all forms of ovarian displacement—namely, that downwards into Douglas's space—the causes of the protrusion are more commonly gynecological, as, for instance, the *vis à tergo* of abdominal or uterine tumours, or the direct tension on the uterine appendages occasioned by displacements of the uterus.

*Symptoms.*—Ovarian hernia manifests itself by the sudden occurrence of a small ovoid tumefaction possessing certain distinctive characteristics, and making its appearance in either the inguinal or femoral regions, or in the labia, or directly downwards in Douglas's space. This tumefaction, as observed in its ordinary condition, is about the size of a large walnut, and when inguinal is usually very slightly sensitive. Before the menstrual periods, however, the extended ovary invariably becomes enlarged—in one instance recently under my care it increased to the size of a small orange—and then gives rise to a dull aching pain, which gradually subsides, so that shortly after the termination of the menstrual epoch the displaced organ resumes its previous condition, and

generally ceases to give any active trouble until its functional activity is again stimulated by the approach of the next catamenial period. In some instances, however, these symptoms do not thus disappear in the interspace, the dull sickening pain remaining permanently, and the congestive hypertrophy of the displaced organ continuing to increase until relieved by suitable treatment.

*Diagnosis.*—That the differentiation of ovarian displacements was formerly very imperfect is, I think, evident from the scant notice of such cases by the older gynæcologists, by whom their existence was either ignored or confounded, when external, with enlarged inguinal or femoral glands, or, when labial, with other tumours in that situation; whilst ovarian protrusion into Douglas's space was apparently in many instances taken for pelvic abscess, subperitoneal pedunculated fibromata, hæmatocele, or the reflexed fundus uteri. We at least have now no excuse for similar errors in the diagnosis of ovarian herniæ. These, whether inguinal or femoral, may be readily distinguished from enterocele by the entire absence of the characteristic smoothness and globular form, gurgling on compression and resonance on percussion of the latter, whilst from epiplocele they may be differentiated by contrasting the firm, clearly-defined ovoid tumour observable, if it be ovarian, with the soft, doughy feeling and irregular, ill-defined outline of the hernia, if omental. From enlarged inguinal or femoral lymphatic the ovarian tumefaction may be recognised by the smaller size and multiple character of the former. From pelvic, psoas, or other abscess, the distinction of an extended ovary is obviously rendered easy by the history of the case, as well as by the presence or absence of fluctuation. Lastly, ovarian prolapsus into Douglas's space is distinguished from a posterior uterine displacement, or a fibro-myoma by recto-vaginal examination and the use of the sound. Whilst from the tumefactions in the posterior vaginal *cul-de-sac* that may be occasioned by cellulitis, rectocele, tubal or parovarian cysts, or abscesses, or pedunculated subperitoneal fibromyomata, ovarian prolapsus may be differentiated by the methods of examination just alluded to, which will enable us to determine not only the existence of any uterine mal-position, but also the character of the tumour and the presence of any fluctuation therein. If the uterus be thus found normal in size and position, if there be no fluctuation discoverable, and if at the same time in the posterior *cul-de-sac* we discover a small, well-defined, firm, ovoid tumour, enlarging regularly at each menstrual period,

and which, on slight pressure, gives rise to peculiarly sickening dull pain, we need have little hesitation in concluding that we have to deal with a case of prolapsed ovary.

*Treatment.*—The treatment of ovarian displacements is necessarily dependent on the situation of the extruded organ in each case; or, in other words, whether it be found at either of the abdominal rings, or in the labium, or in the recto-vaginal interspace. In the first of these, whether the ovarian hernia be above or beneath Poupart's ligament, an effort should in the first instance be made at its reduction by taxis. In the majority of cases, however, such herniæ are irreducible when seen by the gynæcologist, and even in those few instances in which reduction is possible, the retentive pressure of a truss is neither endurable nor effectual. In most cases of this kind we must, therefore, be content to protect the ovary if protruded externally from further extrusion or injury by a well-fitting hollow truss. But before this an attempt should be made to lessen the local hyperæsthesia of the generally hypertrophied displaced gland by sedative applications, and, if necessary, by leeching, &c., whilst the constitutional irritation almost always present in such cases should be allayed by suitable constitutional treatment.

When, however, these measures prove ineffectual in relieving the almost constant, worrying, dull aching pain which at each monthly period in these cases becomes accentuated into acute suffering—when, too, the patient's health is endangered by the gastric disturbance and constitutional irritation occasioned by this apparently trivial and too often neglected displacement—we should then fall back on the extirpation of the dislocated and probably diseased gland as the only resource available under the circumstances.

In my own practice I have more than once been thus obliged to resort to this procedure. In one case the ovarian hernia occupied the right labium, the patient being an unmarried woman about forty years of age, who, when admitted into hospital, was completely broken down by dull, dragging pelvic pain, anorexia, and nausea. She was greatly emaciated, despondent, and hysterical. Menstruation was scanty and somewhat irregular, and physical examination failed to disclose any other local complaint than a tumour, almost as large as a hen's egg, within the right labium, any handling of which occasioned severe pain and nausea. This swelling she had only noticed a few months before admission into hospital. The

patient having been etherised, my colleague, Dr. Kennedy, and myself proceeded to remove the tumour, which was found to extend through the canal, to the walls of which it was firmly adherent in many places up to the abdominal ring, where it tapered off to a narrow pedicle, which was secured by double ligature and divided. In the several adhesions free, general, large vessels had to be secured, and subsequently from the surface a free, venous oozing took place, which was arrested by packing the cavity with styptic cotton, and subsequently bringing the edges together with wire sutures and roller and compress externally. It is needless to add that the operation was thoroughly aseptic. That night she slept fairly; the temperature  $101^{\circ}$  and pulse good. Next morning she had some retching, but was able to retain a little iced champagne and jelly; still she was extremely weak; the temperature was only  $99^{\circ}$  and pulse 120. That afternoon, however, she suddenly became collapsed and died, and we were not able to secure permission for a *post-mortem* examination, which we were anxious to obtain. The tumour removed, on careful investigation, verified our diagnosis, proving to be a greatly hypertrophied and disintegrated ovary.

In the next case of ovarian hernia that came under my observation the issue was more fortunate. In this instance the displacement was situated in the left inguinal region, just above Poupart's ligament; the patient being a young lady who shortly after marriage commenced to suffer from dragging pelvic pain, irritability of stomach, loss of appetite, and consequent wasting and debility, further increased by menorrhagia. She also now became extremely hysterical and despondent, and for nearly two years before I saw her had been under almost continual gynæcological treatment abroad and at home—during this time having, *inter alia*, worn almost every form of pessary for the relief of some supposed uterine displacement. Ultimately a small ovarian hernia, which became very troublesome at each monthly period, was discovered in the left inguinal region, and being then irreducible, after the failure of other treatment, its removal was proposed and agreed to. Accordingly I extirpated the dislocated and hypertrophied ovary, after which she made a rapid and complete recovery.

Last autumn another case of the same kind, occurring in a young unmarried woman, was under my care in St. Monica's ward; but as the patient declined operation at the time, she left, promising to return for this purpose as soon as the hospital re-opened for the

winter session, but up to the present she has not done so. To these cases of true ovarian hernia I might, did time permit, add the history of four or five cases of prolapse of the ovary into Douglas's space which have come under clinical observation in the hospital within the last seven years. In only one of these was vaginal oöphorectomy found necessary, the other being sufficiently relieved by the application of a suitable air-pad pessary.

I need hardly observe that ovarian herniæ requiring removal of the ovary are still more exceptional than cases of the displacement referred to; nor is the performance of oöphorectomy under such circumstances always devoid of risk. Hence, in no instance should this step be resorted to without urgent necessity, and until a fair trial has been first made of other remedial and palliative measures.

In considering the treatment of prolapsus of the ovary, its ætiology must be carefully borne in view. Thus, the extrusion may be due, as already pointed out, to the pressure from above of a uterine or ovarian tumour, or from the traction of a uterine displacement on the broad ligaments, and obviously these abnormalities must be removed or relieved before any successful reposition of the prolapsed ovary can be made. When this condition is due to some accidental circumstance, or to a relaxed state of the parts occasioned by constitutional causes, we may, with greater probability of permanent success, attempt to return and retain *in situ* the displaced viscus. For this purpose the patient—being first etherised in order to permit of the necessary manipulation of the generally highly-sensative and tumefied ovary—should be placed in the left lateral semiprone position, when by firm but gentle, steady, conjoint bimanual pressure through the rectum and vagina, upwards and forwards, we may be able to lift the extruded ovary out of the recto-vaginal fossa and to push it up into its normal position, where it may then be retained by either a Greenhalgh's or Arnold's glycerine pad pessary. When, however, as more generally happens in long-standing cases of this kind, the re-position of the ovary cannot be thus effected, the support of a well-fitting pessary will, in the majority of instances, be found effectual, not merely in preventing any further prolapse, but also in relieving the discomfort and suffering caused by such displacements. And finally, in cases where these measures fail, and where the local and constitutional effects of the ovarian prolapsus are urgent and are otherwise irremediable, we may, as happened in one of my cases, be obliged to resort to vaginal oöphorectomy.

ART. VII.—*Partial Embolism of the Inferior Division of the Central Artery of the Retina, associated with repeated previous Attacks of Chorea.*<sup>a</sup> By ARTHUR H. BENSON, F.R.C.S.I.; Assistant-Surgeon to St. Mark's Ophthalmic Hospital; Ophthalmic and Aural Surgeon to the City of Dublin Hospital; Ophthalmic Examiner, Royal College of Surgeons and University of Dublin.

THE retina offers many advantages not elsewhere obtainable for the observation of pathological conditions during life; and from the changes there observed may be at least conjectured the changes that take place in other somewhat similar structures, more especially the nerve centres.

In the retina we can accurately watch the day-to-day changes that occur, and note the correspondence of objective changes with subjective sensations and physical signs. In no other part of the body can this be done so well, for not only have we the processes directly and clearly under observation through the transparent media, but we have them considerably magnified. The details of the fundus oculi are viewed under a magnification of about 15 diameters. Thus, most minute changes can be noted with certainty. The retina and optic nerve are, moreover, closely allied in structure to the nervous centres, brain, and spinal cord, and they are so intimately associated with the intra-cranial viscera that much may be learned of the intra-cranial conditions by observation of the pathological processes within the eye. It is on this account that I venture to bring this case before the Medical Section:—

CASE.—James J.,<sup>b</sup> aged twenty-one, house painter, applied at the out-patient department of St. Mark's Ophthalmic Hospital on October 15th, 1885 (S. M. O. H. Disp., 2,987). He stated that he had always been shortsighted; that on the previous evening, at seven o'clock, while sitting reading, after an ordinary easy day's work, and while feeling perfectly well, the sight of the right eye rapidly but gradually failed, so that within a few seconds he became totally blind of that eye. He could not even see the light with it, whilst the sight of the left remained perfect. He had no premonitory symptoms whatever, nor any abnormal sensations accompanying the loss of vision. In about three minutes the sight began to clear from below upwards, and gradually improved till,

<sup>a</sup> Read before the Medical Section of the Academy of Medicine in Ireland, Friday, December 18, 1885.

<sup>b</sup> The patient was exhibited.

in about fifteen minutes, the Field of Vision had reached the horizontal line. There it ceased abruptly, and has since remained. The chart taken with the perimeter fifteen hours after the attack shows absolute loss of the upper half of the Field, the boundary of the sentient half being a straight horizontal line coinciding with the horizontal meridian of the chart, except, perhaps, at the fixation point, which appears to include rather more than half of the macula. No headache, giddiness, sickness of stomach, or other symptom either preceded, accompanied, or succeeded the loss or the return of the sight.

The history the patient gave of his previous illnesses is interesting.

1. He had childish illnesses of various kinds. He had measles when three years old, followed by whooping-cough. At nine years of age he had scarlatina, and about twelve he had small-pox lightly.

2. Four and a half years ago, *i.e.*, in 1881, at the age of seventeen, he had rheumatic fever, and was under the care of Dr. Bennett, of Sandymount. This lasted ten weeks. His heart was not, at that time, affected, as far as he could tell. He says that Dr. Bennett considered that lead might have had something to do with the rheumatic fever.

3. Three and a half years ago, June, 1882, he had an attack of left-sided chorea in the Meath Hospital, under Dr. Foot, which was severe for about seven or eight weeks. Dr. Foot has kindly placed the notes of that attack at my disposal. He says:—"There is no cardiac murmur at present (June, 1882). The twitchings had been coming on for six weeks; however, he worked up to the beginning of the week in which he was admitted. The jerkings began in the fingers of the left hand; then spread up to the shoulder; then went to the lower extremities, beginning at the hip; lastly they affected the face—left side—principally the muscles about the mouth and eye. He also felt his neck twitching. He cannot hold anything in his left hand;" but he did not get quite well; and—

4. Two years ago (1883) he had another similar attack of chorea in the Edinburgh Infirmary, and was treated by a physician whose name he forgets. This lasted about two months, and was, he says, more general, affecting both sides.

5. Last year (1884) he had again another attack of chorea, and was treated by Dr. Quinan. This attack lasted about two months. Each attack was less severe than the previous one. He has since had no return.

*Condition on Admission.*—R., with — 6 D. V. =  $\frac{6}{18}$ ; tension normal; pupil active. L., with — 7 D. V. =  $\frac{6}{8}$ ; tension normal; pupil active, but more responsive than in L. This was fifteen hours after the embolism occurred. The ophthalmoscope showed the media to be clear. The upper part of the right disc was fairly normal. There was a myopic crescent and a large deep physiological cup, in which the vein

was pulsating; its lower half was hazy and whitish, so that its margin could not be determined. The retina was divided horizontally by a sharp line of demarcation into a normal superior portion and an œdematous inferior portion. This line ran from about the middle of the disc towards the yellow-spot region passing close to, but just below, the macula. It extended well out beyond the yellow spot, and also to the nasal side of the disc, following, with very great accuracy, the horizontal line. This line was best marked, as was also the œdema of the retina, in that part which lay between the disc and the yellow spot. The retina below the boundary line was of a whitish-gray colour (œdema), fading gradually into tolerably normal-looking retina towards the periphery. On the œdematous portions the smaller vessels stood out with undue distinctness. The macula presented the typical "cherry-red spot," which was horizontally oval; and surrounding it, in the more normal part of the retina, was a most distinct halo. The upper half of the retina exhibited, to a marked degree, the "shot silk" phenomena, but was otherwise perfectly normal. The vessels were all well filled with blood, though those supplying the inferior half of the retina were not quite as roundly filled as those running upwards, but pressure produced pulsation in the arteries of the affected part. Pulsation and emptying were more easily produced in the vessels of the affected than of the healthy retina. There were, however, no attenuated or empty vessels such as are usually seen in cases of embolism of the central artery. There were no hæmorrhages anywhere visible. There was total loss of sensation in the lower half of the retina; even a strong beam of light projected upon that part of the retina was not perceived, whilst the upper half possessed acute vision.

The diagnosis of embolism of the inferior division of the central artery of the retina was made. The urine and heart were each carefully examined at the time and found healthy; and on two subsequent occasions Dr. Hawtrey Benson kindly examined his heart and failed to discover any pathological condition whatever.

In a few days' time the cedema of the retina had markedly diminished. One small flame-shaped hæmorrhage appeared in the retina to the lower outer side of the disc. This was soon absorbed, and no fresh extravasation occurred. The vessels, in a few days, showed a decided tendency to diminish in size, and to exhibit the appearances of thickened coats.

The subjective conditions remained unchanged, but each day the objective appearances varied in proportion as the cedema was absorbed and the vessels shrank, till in a month's time the retina looked practically normal, except for the vessels on its lower half, which were shrunk to a great degree, and had thickened coats. One or two small brilliant yellow spots of fatty degeneration, which were noted in the diseased retina near the yellow spot, had disappeared. The halo round the disc



was still visible, and a faint milkiness of the retina, which had been cedematous, still served to distinguish it from the sound part. Vision remained unchanged, and tension was normal. When the circulation had been re-established, the vessels, more especially the veins, of the affected portion of the retina were smaller on the disc than a short distance away from it.

It is now (Dec. 18th) more than two months since the accident, and the retinal changes above mentioned are only visible to a very slight degree but the vessels, both veins and arteries, are more attenuated, and, vision has in no way changed. Were it not for the history the ophthalmoscopic appearances might be difficult to account for, the only subjective symptom being the horizontal right hemianopsia.

One or two points in this case admit of discussion. Was there any ætiological connection between the embolism, the three attacks of chorea, and the previous attack of rheumatic fever? Acute rheumatism is a common antecedent of chorea; and those who believe in the embolic theory of chorea find in the cardiac complications of rheumatism an explanation of the choreic attack. To such also the sequence would be rendered complete when the individual got embolism of the central artery of the retina; and further probability is given in this case by the fact that it was left-sided hemichorea that he suffered from (*i.e.*, right side of brain), whilst the embolism lodged in the right retina. Cases of this kind are, however, extremely rare. Mr. Swanzy, in the R. L. O. H. Reports,\* has recorded a case where a girl, aged ten, got chorea, and at the same time facial paralysis and embolism of the left central retinal artery. In this case, as in mine, nothing pathological could be discovered in the heart.

In my case the sudden and total blindness of one eye, followed by the recovery, in a few minutes, of half the Field of Vision and permanent loss of the other half, would seem to imply that the embolus, in the first instance, blocked the central artery at or before its bifurcation, and was dislodged from that situation and washed into the inferior division of the vessel before any permanent injury was done to the retina. That this general ischæmia should occur with even a small embolic mass is quite reasonable, as any sudden obstruction, though only partial, would be likely to produce such a diminution of the tension of the blood in the retinal vessels that their coats would spasmodically contract and, aided by the intra-ocular pressure on the vessels, serve to empty them almost to

\* Sept., 1875.

obliteration of their lumen. The mass when dislodged was carried into the inferior division, leaving the circulation free in the superior, and vision in it returned.

The early re-establishment of the circulation in the lower half of the retina, without the restoration of vision, and the subsequent contraction of the vessels and atrophy of the retina, may be explained, as has been done in an able article on the subject by Schnabel and Sachs (*Archives of Ophthalmology*, Vol. XIV., p. 262), by assuming that an embolus of irregular shape only partially filled the lumen of the branch into which it drifted. The spasmodic contraction of the arteries, aided by the intra-ocular pressure, were at first sufficient to complete the arrest of the circulation produced by the partial embolus, but that presently the spasm passing off permitted the blood to flow, though slowly through the vessels, which were thus again filled, but at a lower tension than the normal.

It will be remembered that, as regards its nervous connections, the retina is divided by a vertical line running through the yellow spot and separating it into lateral halves; and hemianopsia from post-ocular trouble is usually lateral, right or left. But as regards its blood-supply it is divided horizontally by a line also passing through the yellow spot. Consequently hemianopsia from vascular causes is usually horizontal, superior or inferior; that the refilling of the vessels in these cases is not due to a regurgitation of venous blood is proved in many instances by actual observation of the direction in which the current flows. I remember seeing one case of recent embolism in which the blood column in the retinal veins was divided into segments, each separated by an apparently empty space. With each beat of the heart these segments gave a jerk forward, towards the disc, and so showed that the circulation, though impeded, was not totally stagnant or reversed in direction. This beading of the blood in the veins lasted only for some hours. When the case was seen next day there was an uninterrupted column of blood in all the vessels. The subsequent contraction of the vessels has been ascribed to secondary changes of a sub-inflammatory nature occurring in their walls, and caused by the partial stagnation of the blood-flow.

Schnabel and Sachs have had the rare opportunity of verifying, by *post-mortem* examination, the diagnosis in a case very like the one I have just described, and I may be permitted to quote their description of the conditions as seen by them:—

"In the central artery of the intra-ocular part of the left nerve, just at the lamina cribrosa, was an embolus, whose long diameter was about twice the transverse diameter of the artery. It was partly hyaline, partly finely granular, and contained no cellular elements.

"It stained an even blue with hæmatoxylin. One part of the upper surface of the embolus had become attached to the wall of the vessel against which it rested; another part projected into the lumen of the artery surrounded by blood. From that part of the arterial wall on which the embolus rested the endothelium grew into the recesses existing between the upper and lower ends of the plug and the arterial walls, and was continued over the upper surface of the plug in a double or triple layer of cells, covering the embolus, and there protecting it from direct contact with the blood.

"The passage for the blood alongside the embolus measured at its widest part about one-third of the diameter of the artery. It was full of normal blood corpuscles, which seemed to form a continuous layer with those above and below the embolus. The two outer arterial coats presented no anomaly in the central arteries. Behind the embolus the lumen of the artery was normal.

"In that part of the lower main branch of the central artery which passed over the papilla was another embolus, which on section was of the shape of a long quadrangle, whose length was 0.16-0.20 mm., and its width 0.025-0.050 mm. Its relations to the lumen of the artery and to its endothelium were similar to the one seen in the trunk of the central artery."\*

If it be true, as Schnabel and Sachs have demonstrated, that "partial emboli" occur in the retina, it is certain that such also occur in other organs, and give rise to symptoms of permanent or temporary importance according to the delicacy of the part obstructed. A very few hours deprivation of blood is sufficient to destroy the function of the retina; but it may be without blood for a certain time and yet recover perfectly, as in the case observed by Wood White (*Ophth. Rev.*, Vol. I., 1882). Portions of the nervous centres may, in the same manner, suffer temporary deprivation of blood without permanent loss of function; and cases of vomiting, headache, giddiness, &c., &c., may in some cases be most easily accounted for by this hypothesis. In this connection it may be mentioned that vomiting and headache

\* Loc. cit., page 277.

are not infrequently associated with the occurrence of retinal embolus.

I have not attempted to discuss the points of diagnosis between embolism and thrombosis of the retinal artery. The subject has recently been treated in full by Priestly Smith (*Ophth. Rev.*, Vol. III., p. 1, 1884), and my case admits of no doubt.

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ART. VIII.—*The Treatment of Acute Traumatic Tetanus by Curare.*

By J. S. M'ARDLE, F.R.C.S.; Lecturer on Surgery, St. Vincent's Hospital.

THE persistent manner in which death defeats our best efforts for the relief of acute traumatic tetanus is evidence of the fact that (notwithstanding the volumes which have been written on the subject) little, if any, advance has been made in this department of medicine. My excuse, then, if excuse be needed, for bringing forward the following case is, that its result, I think, justifies me in looking on curare as exceptionally useful in the treatment of tetanus. That our knowledge of the treatment of tetanus is still deficient is not to be wondered at, since the pathological condition present in this affection is so little understood that no two authorities agree as to the exact lesions found after death. Obscurity surrounds alike the embryonic tissue described by Devine, as engaging the cord and nerves, and the exudation material which Dickinson has met with. Equally insufficient is the hyperæmia and granular degeneration observed by Benedikt, the softening of the gray substance described by Lockhart Clarke, the simple hyperæmia of Gimelle, the sympathetic irritation of Andral, and the reflex irritability of Romberg. A varicose condition of the vessels of the gray substance of the cord has been noticed by Froriep, but that, too, like the other conditions noted, has been of so infrequent occurrence that it cannot be looked upon as more than a coincidence. Heiberg's idea that blood-poisoning affecting the muscles is the cause of spasm is sufficiently disproven by the fact that the conditions most unfavourable for the development of bacteria have not the slightest influence in preventing tetanus.

That neuritis is an important factor in the production of tetanus no one will doubt, and Lepelletier discovered well-marked ascending neuritis in the cases which he had an opportunity of examining, and therefore concludes that the spasms result from an inflammatory condition of the cord. From the foregoing it can be seen

that the microscopic appearances so seldom agree that no definite conclusion can be drawn therefrom as to the real seat of disease; and so, in endeavouring to treat symptoms, every drug which is supposed to offer the slightest resistance to nerve irritation has been tried. During the past few years I have observed accurately the course of four cases of acute traumatic tetanus, and have had an opportunity of testing the value of nicotin, aconitin, morphin, Calabar bean, chloral hydrate, and indeed most of the drugs generally used in this affection, and I must say that in my hands they have signally failed. In my last case I depended entirely on curare, and the result has been most satisfactory. Mine is only one of numerous cases treated by curare, but I have no intention of reviewing them; the record would prove profitless as well as uninteresting—profitless, since the few treated by curare alone would not offer sufficient grounds for drawing conclusions as to its value; uninteresting, since most of them are evidence of erratic administration of the drug, and therefore misleading.

CASE.—The case to which I desire to call attention is that of ——— Murphy, aged twenty-one years, admitted into St. Vincent's Hospital suffering from strangulated inguinal hernia. His general health was good, and as he was anxious to have the hernia cured, I cut down on the external abdominal ring, stitched the neck of the sac with fine chromosised catgut, and brought the walls of the inguinal canal together with strong catgut. Everything went well until the tenth day after the operation, when he complained of pain and tenderness over the lower part of the abdomen and difficulty in opening his mouth. On the twelfth day after the operation the hernia came down during a spasm of the abdominal muscles; his condition on the 13th was one of extreme distress. During the previous night he had bitten his tongue very severely several times, and slept badly. His condition was as follows:—His abdomen was hard and tympanitic; dorsal muscles powerfully contracted; patient resting on occiput and heels; upper eyelids drawn down; eyebrows raised; pupils greatly contracted; face wore an anxious expression, and he complained very much about his tongue, which was swollen and deeply indented by the teeth, which could only be separated a quarter of an inch; breath very offensive; all the voluntary muscles were in a state of spasm, the slightest noise or disturbance increasing their rigidity and rendering the patient extremely miserable. There had been no evacuation of the bowels for three days, notwithstanding the fact that calomel in ten-grain doses had been given each day, and bromide of potassium and chloral hydrate regularly. This state of affairs lasted for two days longer, and the patient was almost exhausted,

when I injected one-half a grain of curare with the following effect:—Immediately after injection dyspnoea set in, and he complained of uneasiness in the cardiac region; his pulse became uncountable, his face purplish, and his pupils dilated. He complained of dimness of vision and of seeing double. In seven minutes after injection the dorsal muscles relaxed; the abdomen became slightly soft; perspiration broke out over his face and chest, and, sinking down in the bed, he fell into a sound slumber, which lasted three hours, during which time he was not disturbed by spasms. In the fifth hour the bowels were evacuated, and it was only at this time that spasms began to reappear. In the sixth hour he had another dose of curare, which acted as well as the first. Purgatives of every kind were avoided, and yet the bowels acted regularly; and although the patient occasionally suffered a little, he was in a state of comparative comfort from the time I commenced the curare until the spasms disappeared. I noticed that as long as his pupils remained dilated, spasms, if they occurred at all, were very slight, and this dilatation rarely passed off until four or five hours after injection. Consequently I ordered  $\frac{1}{4}$  gr. to be given every sixth hour, with the result that the patient suffered very little until he was quite restored to health. He was three weeks under treatment, and the effects of the curare—as dilatation of pupils, quick pulse, incoordination of muscles—lasted for six days after the injection was omitted. The slight variations from day to day only show that the urinary secretion increased after the first few days, at which time the patient complained of thirst. For the first few days after injection there was a considerable flow of saliva, mixed with pus and mucus, and the discharges from the bowels were very dark and foetid.

It will be noted in the above case that in from 6 to 10 minutes after the injection of curare the muscles became relaxed, with the exception of the abdominal ones; respiration became laboured, lips cyanotic, pulse very rapid, pupils widely dilated, and that in no instance did more than 10 minutes elapse after injection without the most marked relief being obtained. An important point in connection with this case is the fact that, under the influence of curare, the bowels were evacuated regularly, although constipation was the rule before its administration. This I look upon as evidence of the absence of tetanic spasm of smooth muscles and of the power of curare to keep the sphincters in a state of relaxation.

Now that I have noted the effects of curare in this case of more than ordinary gravity, I am desirous of calling attention to the method of administering the drug. Since it loses all its power when taken into the intestinal tract, we have only to consider its

action when employed epi-, en-, and hypo-dermically. When applied to unbroken surfaces it has little or no effect, and hence its use in this way is now completely abandoned. The endermic method is also unpromising, as the varying conditions of the surface to which it is applied cause the action to be very uncertain. Hypodermic injection requires care, as, even when used in this manner, the drug may act inefficiently. In looking over the history of cases unsuccessfully treated by urari, one is struck by the fact that failure (in the majority of them at least) depended rather on the method of administration than on the inactivity of the drug. The rules which should guide us in the use of curare are:—

I. If a watery solution be used it is necessary to dissolve a fresh supply every few days, as the active principle with some resinoid materials precipitates early.

II. In injecting, the needle should be passed horizontally under the skin, so that rapid action may be avoided; in my case, whenever the needle was allowed to enter deep structures the effects of the drug became alarming.

III. The dose, to be of service, must be large, and frequently repeated— $\frac{1}{4}$  to  $\frac{3}{4}$  grain every fifth hour; some even go so far as to state that  $\frac{1}{2}$  grain is the proper dose for an adult.

IV. The solutions should be filtered, else very troublesome abscesses form at points of injection; such abscesses occurred in the early part of my case.

Some few writers advocate the use of small doses; the history of their cases, however, shows the worthlessness of this opinion, since the ones which have not succumbed under treatment by small doses were exceptionally mild, and should not be classed amongst cases of acute traumatic tetanus.

One great difficulty in connection with the treatment of tetanus by urari lies in the fact that no two specimens of this drug agree in strength, and hence experimental injections (into rabbits or dogs) should be made before using a fresh sample. This is, of course, almost an impossibility in this free country, and so, in my case, I was obliged to proceed cautiously at first; the necessity for so doing became apparent immediately after the first dose, as noted in the history of the case. This difficulty, as well as the doubt concerning the source of the drug, has led to the discovery of its active principle by Boussingault, who obtained it as an amorphous powder soluble in water, and having the same action as

the entire drug, much smaller quantities being required to produce powerful effects. Preyer has been able to isolate a crystalline substance to which he gives the name "curarin;" he describes it as a very hygrometric crystalline substance, containing no oxygen, becoming brown on liquifying in air, capable of forming salts with several of the mineral acids. These salts crystallise readily, are very deliquescent, freely soluble in water, their solutions possessing much more powerful poisonous properties than curare, and not decomposing when kept for considerable periods. The substitution of such salts for crude urari would be attended with good results, since in employing them we could be certain of the strength of the solution in use, and the necessity for preliminary experimental injection would be avoided.

The amount of cardiac distress produced by curare is an objection to its use; the heart beats become innumerable owing to the paralysing effect of the drug on the inhibitory fibres of the vagus. It is possible this trouble may be overcome by using nitrate of pilocarpin, which has an opposite effect on the cardiac nerves.

The fact that we are not acquainted with a reliable antidote to curare is also objectionable. When the Indians desire to prevent its action they immerse the animal in water and give it large quantities of rum, or they administer the juice of the sugar-cane. To keep up artificial respiration is probably the only thing we can do, unless, indeed, ligature or amputation above the point of entry of the poison. Carbolic acid is a powerful antagonist to the drug, but the difficulty of introducing it without injurious effect renders it valueless. Pilocarpin, although possessing an opposite action on the heart, will scarcely counteract the ill effects of curare. Dr. Berckhan has made an interesting communication to the *Berliner klin. Wochen.* (November 30th, 1885), dealing with this subject. He brings forward a case which had been under treatment with chloral hydrate in large doses from the 8th to the 24th March without any improvement—in fact, the patient was becoming exhausted, when, on the 24th, after experimental injection into a dog, he commenced the treatment by curare. On the 28th, after the administration of  $4\frac{1}{2}$  grains, the spasms had entirely subsided, and without further medication the patient made a good recovery. This case was not so acute as mine, and possibly the exhausted state of the patient rendered less curare than I have recommended sufficient. Dr. Berckhan gave it only twice in 24 hours, and in doses of half a grain. I gave three-quarter grain



every sixth hour. The latter would, undoubtedly, be too large to begin with in any case, and I think much too large for a weak subject. My patient was a powerfully-built fellow; and although the first injection produced rather alarming symptoms, he showed no sign of distress, when the dose was afterwards raised to  $\frac{3}{4}$  gr., and he took in all 45 grains of the drug.

Dr. E. Goutermann (*Berliner klin. Wochen.*, Oct. 29th, 1883) reports a case in which curare had the most desirable effect in a very severe case of acute traumatic tetanus. He is of opinion that large doses must be used if we are to derive the full benefit of the curare treatment; and he mentions a case treated by Dr. de Grousilliers which ended fatally, although curare was used; but the fact that the doses were infinitesimal ( $\cdot 01$  gr.) is sufficient to show that the unfavourable result was not owing to the inactivity of the drug but to its insufficient supply.

Dr. Kræg (*Langenbeck's Archiv. für. klin. Chir.*, 1883, p. 338) notes four cases in which death occurred, notwithstanding the copious display of curare. The cases, however, were exceptionally severe, and it is doubtful whether the result is or is not due in part to curarisation, since the dose (1 gr. in the hour) seems very large.

ART. IX.—*Leamington as a Winter Health Resort.* By FRANCIS WILLIAM SMITH, M.D., Author of "the Saline Waters of Leamington."

IT is impossible to exaggerate the importance of embracing the favourable opportunities which present themselves to individuals and communities. "Opportunity makes the thief," and opportunity makes the merchant.

"There is a tide in the affairs of men  
Which, taken at the flood, leads on to fortune."

There can be no question that the present is Leamington's golden opportunity. English spas are rising in public favour. Many physicians in London and the provinces, instead of exposing their patients to the annoyance and fatigue of foreign travel, are very prudently recommending them to use our own mineral springs, and it cannot be too often repeated that there is now no need for invalids to run the risks and undergo the inconveniences and discomforts of a journey to the Continent in order to drink mineral waters. We have as good saline springs in Leamington as there are anywhere.

In our "temple of health," those who are sick may rest assured of finding relief and often a cure in many of "the ills that flesh is heir to," and it will be well if patients, who are wearied and worried with the rush to Baden-Baden, Homburg, and Kissingen, and the fleeing and flaying experienced at these places, would take this to heart.

Leamington, like most places, has a history. True it is that it cannot boast of the antiquity of some health resorts, nor, like Royat and Dax and half a dozen other Continental spas, claim that Julius Cæsar was cured of his rheumatism by frequenting its baths and drinking its waters; but still, it would appear that the healing virtues of our mineral waters have been known and appreciated in the immediate neighbourhood, and even throughout the midland counties, for hundreds of years. As far back as 1586 mention is made of the saline springs of Leamington by Camden, and from that time to this they seem to have had "their ups and their downs." Who is there that has not heard of the great Dr. Jephson, who reigned supreme here for half a century, and attracted thousands from all parts of England, Scotland, and Ireland, nobles and commoners, gentle and simple, by his magic power! The caprice of fashion, and possibly the facilities afforded for travel by steamboats and railways, have drawn people away to the Continent, but we have every reason to believe that there is now a backward wave in our favour.

Royal Leamington Spa, including Milverton and Lillington, with a joint population approaching 30,000 inhabitants, is situated nearly in the middle of "leafy" Warwickshire. The borough stands at 190 feet above the sea-level, and its situation is of great natural beauty. It is often pronounced the prettiest and cleanest town in England, and by this appellation it is well known in the United States. The greater part of the town is built upon a regular plan, with wide and open streets, which in many instances are bordered with chestnut and lime trees, and these impart to the place the aspect of a German spa.

Hygienically considered, Leamington stands well, and for the number of its population ranks as one of the healthiest towns in England. It is blessed with an abundant supply of pure water, derived from an artesian well, of which it is said by Dr. Tidy that "as a dietetic water it is in every way of excellent quality, and for wholesomeness leaves nothing to be desired." The borough has a pattern system of drainage. The sewage matter is pumped up to a

farm, situated some miles from the town, and is considered a model of its kind. The sewers have numerous ventilators and are frequently flushed, thus insuring a healthy sanitary condition of the houses. All these matters of detail help to make Leamington a high-class health resort; and when I say that the death-rate from all causes for the last three years averaged only 15·24 per 1,000, and that from zymotic diseases during the same period 1·1, I think it will bear favourable comparison with other inland watering-places of equal population. The climate and temperature of Leamington may be described as equable, and not subject to great extremes. The annexed tables of comparison will give the reader an excellent idea of its general meteorological bearings. The former embraces a period of ten years, from 1874 to 1883; the latter covers an equal space of time, but not the same years:—

METEOROLOGICAL TABLE.

Places			Inches	Rainy Days	Observers
Leamington	-	-	30·2	176	Jones, Whish, Field, Barnitt.
Great Malvern	-	-	32·1	168	Sandoe, Palmer, Mann.
Cheltenham	-	-	32·3	172	Mackgill, Landsboro, Kay, Humphries, Kynaston, Tyrer.
Harrogate	-	-	33·5	161	Coupland.
Tunbridge Wells	-	-	34·0	158	Miller, Townhend, Winton, Brentnall, Buchanan, Siemens, Royal Meteor. Soc.
Bath	-	-	35·3	180	Barter, Russell, Stokes.
Clifton	-	-	39·1	177	Burder.
Ilkley	-	-	39·9	200	Dymond, Hainsworth, Call, Scales, Richardson.
Matlock Bath	-	-	41·2	176	Chadwick.
Buxton	-	-	53·4	196	Sykes.
Average	-	-	37·1	176	

	Deg. Fahr.
Buxton	45·2
Leamington	48·0
Cheltenham	48·3
Clifton	48·7
Torquay	49·8
Bath	50·3
Bournemouth	50·3
Llandudno	50·5

Loudon says:—"Leamington, from being situated at a distance from the coast, and in the midst of a level country, is exposed neither to sudden gusts of wind nor to frequent rains, which a mountainous neighbourhood so constantly attracts. The temperature is more equal than that of the greater part of the inland watering-places in the country. The highly cultivated state of the soil around the town, entirely free from morasses, with the numerous scattered woods and rivulets, contributes to its being one of the most salubrious spots in the inland counties."

The climate of England is well known to vary and to be uncertain, no matter in what part people live. That of Leamington will seem, from the foregoing tables and quotation, to be one which well admits of its being a place of permanent residence. The fact that it has become so speaks for itself. And certainly, so far as taking a course of the saline waters goes, this may be begun and carried on at any time of the year, which cannot be done in the more northern spas of Scotland and England, or in nine-tenths of the Continental ones.

Dr. Hermann Weber, who is the greatest living authority on mineral spas, observes: "We can see no reason why Leamington should not again become a much frequented and useful watering place, the more so as in many instances it may be resorted to in *early autumn or even winter*, on account of its great mildness of climate compared with Continental spas." Although in the spring, summer, and early autumn, patients look for most benefit because they can bathe without the same risks of taking cold, still, there is no doubt but that more come to Leamington in the winter now than formerly, for "courses" of the mineral water and for the baths. It is seldom they cannot find three or four days in a week genial enough to bathe in, and in this respect it is more like Bath than any other British watering-place.

I have quite recently had an analysis made of all the mineral waters at present in use at the Royal Spa, but as all are saline and more or less alike, I think my object will be gained by giving that of two—namely, the Pump Room and the Chalybeate Wells.

The following is an analysis of the *Pump Room Well*, by Dr. Meymott Tidy, M.B., F.I.C., F.C.S., Professor of Chemistry, Medical Jurisprudence, and Public Health, the London Hospital, and Analyst for the City of London, made March 20th, 1885:—

					Grains per pint.
Sodium	-	-	-	-	36·35
Magnesium	-	-	-	-	2·33
Calcium	-	-	-	-	7·52
Chlorine	-	-	-	-	56·15
Sulphuric Acid	-	-	-	-	25·05
Peroxide of Iron	-	-	-	-	·25
Silica	-	-	-	-	·14

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 127·79

*Chalybeate, the Public Fount.*—Analysis by Professor Brazier, made April 25, 1885. Chief gas is carbonic, with small quantities of oxygen and nitrogen. Alkalinity expressed as carbonate of calcium is equivalent to 1·12 per pint:—

					Grains per pint.
Sodium	-	-	-	-	32·67
Magnesium	-	-	-	-	1·53
Calcium	-	-	-	-	5·18
Chlorine	-	-	-	-	45·12
Sulphuric Acid	-	-	-	-	25·74
Carbonate of Iron	-	-	-	-	1·01
Carbonate of Calcium	-	-	-	-	·67
Carbonate of Magnesium	-	-	-	-	·03
Silica	-	-	-	-	trace.

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 111·95

By combination, the following salts are distributed in these wells in varying proportion:—

Chloride of Sodium.	}	Traces.
Sulphate of Sodium.		
Sulphate of Magnesium.		
Sulphate of Calcium.		
Chloride of Calcium.		
Carbonate of Calcium.		
Carbonate of Iron.		
Chloride of Magnesium.		
Carbonate of Magnesium.		
Bromide of Sodium.		
Chloride of Potassium.	}	
Carbonate of Sodium.		
Lithium.		

We have, in these waters, mineral salts which we can recommend as powerful remedies in the cure of certain diseases. They are such that we can, with a certain amount of nicety, explain their action. This is most important in mineral waters. They have a fixed strength, and we are thus enabled to prescribe them with accuracy. This cannot, I fear, be said of all mineral waters. It will be observed, on looking at the analysis, that the bases of the salts seem to tend in one direction so far as the diseases go for the relief of which they are indicated. The chief bases—sodium, calcium, and magnesium—are all alkalisers of the blood, and as such are most useful in gout, rheumatism, gravel, and in a disordered state of digestion arising from excess of acidity.

Taken warm at the source, in the early morning, in quantity from ten to twenty ounces, succeeded by a gentle walk, and it may be a cup of tea or breakfast—the effect upon most people is a smart liquid purge, with a feeling of relief of weight from the abdominal cavity. When taken in less quantity, and it may be diluted with drinking water, the action is more of a diuretic kind. The “all-round” action, therefore, of Leamington salines is purgative and diuretic; they are alkalisers and general blood-purifiers. In dealing with a few of the diseases in which I find them most beneficial, I will, in as few words as this short paper will allow, run over the principal heads and modes of action in each. Take irritative dyspepsia, where there are fermentations of acids in the stomach, with constipation, eructations of gases from the same organ, palpitation of the heart, and other disturbances—the Leamington salines remove matters from the alimentary canal that are a source of irritation; they prevent an accumulation of such matters as produce disturbances in other organs; and they likewise neutralise the ferments of indigestion, and remove excesses by purgation and diuresis. In congestion of the kidneys they are most valuable. Independently of the “communications” anatomically, by which the kidneys may be relieved by purgation through the portal circulation, there is a much more important physiological relation between the two excretory organs (bowels and kidneys); for if the Leamington salines fail to purge, or purge but in part, they pass on to the kidneys, and act as powerful diuretics. They are chiefly alkaline in their influence on the blood and urine, but are also independently active as specific renal stimulants. In congestion of the liver a course of the Leamington salines relieves the engorged portal circulation, which lies at the circumference and between the lobules

of the liver. At the same time, doubtless, some of the salts are absorbed into the blood, and excreted by the kidneys, which they powerfully stimulate, and thus open up the urinary discharge, which is the second great channel of relief to the liver. In gout which has become chronic, Leamington salines are most beneficial. The excesses of urates and uric acid in the blood are eliminated from the system by a well-directed course of the mineral waters. In this way gout is relieved, and in many cases for the time cured, by the uric acid being alkalisied by the sodium, magnesium, and calcium salts, and by the channel of excesses being diverted through the bowels and kidneys. In the same way chronic cases of rheumatism are relieved and cured when ordinary remedies have failed. Themselves alkalisers of the blood, they act upon the plasma indirectly by combining with the rheumatic acid, and carry it out of the system by virtue of their diuretic influence.

Sciatica, when mixed up with the gouty or rheumatic diathesis, is frequently cured by these mineral waters—so too eczema, psoriasis, and many other skin diseases. In cases also of anæmia I have seen most marvellous benefit derived by their use and the addition of iron to the chalybeate water. Space will not allow me to reason out the *modus operandi* of the Leamington salines in this disease, and the reader is referred to my work upon the subject; nor can I now explain how hæmorrhoids, pelvic congestions, struma, climacteric disorders, paralysis of some kinds, and degeneration of tissues and vessels, all more or less find antidotes in these salines. But such is the case. The baths of various kinds, which may be had at the Royal Pump Rooms, materially assist the internal administration of the waters. These are of various kinds—douche, saline, vapour, Turkish, &c., &c., and are in operation all the year round. The Corporation are at present applying to Parliament for powers to borrow £20,000, which it is purposed to spend in additions and improvements, from time to time, so as to make the bathing establishment second to none in the kingdom.

But time and space request me to stop, so all that I wish to press, in addition, is the following table, showing the comparative composition of the Leamington waters with those of similar saline and carbonated saline waters in some of the popular German and French spas (expressed in grains per pint) :—

TABLE.

SPA	SOURCE	Sodium Chloride	Magnesium Chloride	Calcium Chloride	Sodium Sulphate	Calcium Sulphate	Magnesium Sulphate	Calcium Carbonate	Magnesium Carbonate	Iron Peroxide	TOTAL	
LEAMINGTON	- Pump Rooms -	84.8	.49	—	—	20.66	7.44	.78	.04	.08	114.3	{ With traces of bromide of sodium, chloride of potassium, carbonate of sodium and lithium.
	- Grove's Well -	92.01	1.02	1.85	—	21.87	11.52	1.58	—	.25	129.96	
	- Ayleford Well -	68.68	—	—	12.82	17.58	7.20	.39	.15	.05	106.82	
MARIENBAD	- Public Fount -	74.35	—	—	10.68	17.61	7.65	.67	.08	1.01	111.95	
TARASP	- Kreuzbrunnen (Bohemia)	16.32	—	—	47.55	—	—	4.98	4.16	.33	85.18	
	- Great Spring (Lower Engadine) -	36.70	—	—	20.67	8.77	—	22.86	9.60	.25	141.55	{ With bicarbonate of sodium in varying proportions.
FRANZENBAD	- Salzwelle (Bohemia)	11.25	—	—	22.50	—	—	2.86	—	.02	47.88	
ELSLEB	- Marienbrunnen (Saxony)	17.87	—	—	28.25	—	—	1.37	—	.43	55.06	
ROHITSCH	- (Styria)	—	—	—	18.75	—	—	13.75	11.25	.07	51.32	
BADEN	-	2.48	2.01	—	2.65	7.06	5.62	1.62	—	—	17.46	
KISSINGEN	- Ragotz (Bavaria)	55.88	2.91	—	—	3.73	5.62	10.17	—	.30	81.54	{ With small quantity of chloride of lithium.
	- Pandur -	52.98	2.02	—	—	2.87	5.73	9.73	—	.25	76.04	
	- Maxbrunnen -	21.90	.63	—	—	1.32	2.27	5.77	—	—	33.31	
HOMBURG	- Elizabethenbrunnen (Nassau)	94.6	7.0	6.6	—	.12	—	14.5	.25	.22	128.49	{ With chloride of potassium.
	- Kaiserbrunnen -	68.8	4.0	5.25	—	.12	—	8.8	.37	.22	89.36	
WIESBADEN	- (Nassau)	65.61	1.85	4.5	—	.86	—	4.01	—	.05	78.30	
SODEN	- (Nassau). Milchbrunnen	22.1	—	—	—	.24	—	3.41	1.71	.20	29.32	
	- Warmbrunnen	32.66	—	—	—	.31	—	5.58	3.23	.37	44.1	{ With chloride of potassium.
	- Wilhelmbrunnen	130.1	—	—	—	1.22	—	10.47	1.60	.37	147.4	
	- Soolbrunnen	143.0	—	—	—	.95	—	10.78	.36	.75	161.97	
ARNSTADT	-	32.62	.62	—	1.90	4.05	.90	1.25	.05	—	41.42	
BOURBON LANCY (Saône-et-Loire)	-	11.37	3.5	.43	2.18	.17	—	.52	1.31	.17	19.32	
BOURBONNE-LES-BAINS (Haute-Marne)	-	50.75	3.5	—	—	7.70	—	.87	—	.02	66.72	
SALINS-MOULIERS (Savoie)	-	99.02	—	—	5.6	12.18	6.58	8.79	—	—	132.48	
BRIDES-LES-BAINS (Savoie)	-	10.69	—	—	9.02	20.56	6.12	2.84	—	.14	49.73	



In conclusion, I think it will not be out of place to let the profession know, in the interests of patients, that in Leamington will be found every kind of accommodation suited to the sufferers' wants—spacious and comfortable hotels, lodging-houses, sunny, well appointed, and clean—to say nothing of noiseless and easy bath-chairs and all manner of invalid appliances. For those who are well enough to enjoy out-of-door exercise, there is all through the winter months the noble sport of fox-hunting, which they may follow to their hearts' content. Leamington has for many years been the rendezvous of "mighty hunters." The country around, too, is fertile, undulating, and wooded, and abounds with objects of the greatest historical interest, for within easy access are Shakspeare's birthplace, home, and final resting-place at Stratford-on-Avon; Warwick Castle, one of England's noblest piles; Kenilworth Castle, a stately ruin associated with royal gatherings, silent gondolas, splendid tournaments, gorgeous banquets, and burly Roundheads; and other places of equal historical interest, such as Edgehill, Stoneleigh, and Coventry with its three church spires.

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#### AFFECTIONS OF THE EYE ACCOMPANYING MUMPS.

THE implication, either concomitantly or by a process of metastasis, of other and distant organs during an attack of mumps, is a well-known peculiarity of that disease. The method of this transference is one of the many questions pertaining to that obscure disease that yet remain to be solved by the general pathologist. The testicle in man, and the mammary gland in women, have been recognised from the earliest history of medicine as the objects of this metastasis of disease from the parotid glands; and within late years the ear has been discovered to stand in a similar unfortunate relation. That another important organ of sense was in danger from this usually mild and innocent affection, is now pointed out by Dr. Swan M. Burnett in the January number of the *International (American) Journal of the Medical Sciences*. From a study of the cases reported up to the present time, he finds that the principal parts of the eye to suffer from a metastasis of the mumps to that organ are the lid, conjunctiva, and optic nerve, and also in his case the third pair of nerves. The disease may be unilateral and of varying degrees of intensity. The prognosis of the affection seems to be, in the main, good. This, however, suggests the possibility that some of those atrophies of the optic nerve, especially when unilateral, which are accidentally discovered, and the origin of which cannot be traced to any of the hitherto recognised causes, may be due to a metastasis of mumps in childhood.

## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

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*History of Homœopathy: its Origin: its Conflicts. With an Appendix on the Present State of University Medicine.* By WILHELM AMEKE, M.D. (of Berlin). Translated by ALFRED E. DRYSDALE, M.B. (of Cannes). Edited by R. E. DUDGEON, M.D. London: E. Gould & Son. 1885. Pp. 445.

THIS is a dreary book, not likely to be read by anyone but conscientious reviewers (like ourselves) and professors of the Hahnemannian Cult. If it were more controversial it would be more interesting, and might even be amusing. There might be some pleasureable excitement in the sensation of being on the brink of perversion to the belief in infinitesimal doses; but, though this book is the product of three homœopathic brains—written by Dr. Ameke, translated by Dr. Drysdale, and “edited” by a high priest of homœopathy, Dr. Dudgeon—it is not calculated to affect medical opinion one way or the other. It is a “history of homœopathy;” and the editor opens his preface with the solemn pronouncement—“The history of homœopathy is the indictment of the medical profession;” yet, somehow, we don’t feel indicted. The author composes a long panegyric on Hahnemann, and pours scorn on that worthy man’s enemies (who, truth to tell, often deserved it); but neither process affects in the slightest degree the questions on which physicians<sup>a</sup> and homœopaths are at issue. Hahnemann may have been an expert chemist and pharmacist; he may have clearly seen and sturdily opposed the vices of the medical practice of his day; his method of ascertaining the power of drugs by experiments on healthy subjects may have been excellent; he may have written

<sup>a</sup> One of the extremely few things to be found in Dr. Ameke’s book is his evident delight in the use of that wonderful word “allopath”—word of unknown meaning and mysterious etymology—to all who do not swallow the Hahnemannian globules of dogma. It seems as soothing to him as “that blessed word Mesopotamia” to the old lady in church. We protest not, nor complain. The gigantic navy beaten by his tiny wife replied to wondering expostulation:—“Wy, it amuses ’er, and don’t do me no manner of ’arm.”

114 works of various sizes and values; but the demonstration of any or all of these propositions brings us no nearer to demonstration that *similia similibus curantur* is a therapeutical principle of universal application, or that a finite quantity infinitely divided may *not* be equal to zero. On the other hand, Hahnemann's absurdities on the subject of itch do not in any way affect the soundness of the homœopathic principle or the question of the efficacy of infinitesimal doses. The latter point, we may observe here, has probably been given up in practice by the modern homœopath. Dr. Dudgeon avoids it in his preface, and we have observed no allusion in the text to the charge which has been made—and maintained—that the disciples of Hahnemann at the present day have abandoned the Hahnemannian posology.

Hahnemann's character is plainly enough revealed to us in these pages; but the main features of it are familiar to everyone who has seen the plaster busts which adorn the windows of homœopathic druggists. What friends call firmness and enemies obstinacy, profound self-confidence, and contemptuous pity for the unhappy beings who differ from him, are stamped upon the page as upon the cast. It is clear that he was what Anglo-Indians call "fighting-caste;" and that if he had been less quarrelsome, dogmatic, and uncompromising, he would have served his own purposes better, and better helped the cause of scientific medicine. One of his panegyrists, indeed, transfers the blame for his fractiousness to other shoulders. The bitterest "allopathist" will scarcely deny the tribute of an infinitesimal tear when he reads the following passage from Brunnow's "Ein Blick auf Hahnemann":—

"Strict as was the obedience Hahnemann demanded from his children, as a husband he was far from having the rule in his own house. His tall and stout wife, who, as Agnes Frei did to the noble painter, Albrecht Dürer, gave him many a bitter hour, exercised a most baneful influence over him. It was she who cut him off from society, and set him against his medical colleagues. It was she who often caused dissension between himself and his most faithful pupils, if they did not treat the doctor's wife with the deepest respect. Notwithstanding this, Hahnemann was accustomed to call this scolding Xantippe, who took pleasure in raising a storm in the house, 'the noble companion of my professional life.'"

Undeterred by past experience, Hahnemann, at the age of eighty, married "a highly cultivated French lady" of thirty-four, who had visited Cöthen to place herself under his medical treatment.

She induced him to settle in Paris, where he was well received, where he passed the last eight years of his life, and where he died in 1843.

The facts brought forward by Dr Ameke establish abundantly Hahnemann's claims to the character of an expert and scientific chemist. At an early period of his career he turned his attention, with valuable results, to the detection of adulteration of drugs. It was in connection with this subject that he made the important discovery that sulphuretted hydrogen precipitates certain metals from acidulated solutions, leaving others in solution. The application of this discovery to the detection of lead in wine was the first use to which it was turned by the discoverer; but, we need scarcely say, it has since proved of immense importance in the laboratory. As an example of the condition of physiological chemistry in 1810, the author instances a work on "Spontaneous Combustion," which appeared in that year. "The sudden ignition of the human organism and its combustion with the appearance of flames, so that only ashes or coal—in one case only a spot of grease—remained of the whole body," are explained satisfactorily as follows:—

"1. The whole body of the consumed persons was penetrated through all its cells by hydrogen gas—at least in sufficient quantities to suffice for its first ignition and the maintenance of the fire. 2. An excess of other inflammable matters—as sulphur and phosphorus—was simultaneously present. 3. The body, thus in a high degree inflammable, was not ignited by any external fire, but by an electric explosion in its interior; the electric spark quickly permeated the body filled with inflammable matter."

Inorganic chemistry was at this time in a similarly crude condition, and Hahnemann was one-eyed amongst the blind. How he came to be a physician is not made very clear in these pages; but in medicine, as in chemistry, he was, in some respects, in advance of his contemporaries. For instance, in one of his minor works, published in 1796, he says:—"I never allow an insane person to be punished, either by blows or any other kind of corporal chastisement, because there is no punishment where there is no responsibility, and because these sufferers deserve only pity, and are always rendered worse by such rough treatment, and never improved." Again, we think Dr. Ameke is justified in claiming for his client the credit of having been the first to test systematically by experiment the action of medicines. In 1805 he published his "*Fragmenta de Viribus Medicamentorum Positivis*," a work of 740 pages, in which he details

the results (partly obtained by experiment upon himself, partly from toxicological observations) of his studies of the action of twenty-five drugs. It was in the course of these researches that the Homœopathic law, as he deemed it, occurred to him; and we find it first stated formally, in 1796, in an "Essay on a New Principle for Discovering the Curative Power of Drugs," which appeared in *Hufeland's Journal* in that year. The principle is laid down in the following words:—"We should imitate nature, which sometimes cures a chronic disease by superadding another, and employ in the (especially chronic) disease we wish to cure that medicine which is able to produce another very similar artificial disease, and the former will be cured; *similia similibus*." Even Dr. Ameke admits that "Hahnemann here commits a great error—the greatest possible under the circumstances"—in founding his law on an insufficiently wide induction. "He leaves the method by induction too soon, and assumes the truth of many effects of drugs which he should first have tested. Various hypotheses are quoted instead of evidence, while other examples are very unsatisfactory" (p. 107).

Modern homœopathy would seem to be standing in a state of unstable equilibrium upon its *similia-similibus* leg, the other—infinitesimal posology—having been knocked from under it by experience and common sense. We can understand how Hahnemann, with a constitutional tendency to reaction, and with the habits of an investigator, should have revolted against the coarse medication of his day and carefully tested the efficacy of diminished doses; but how a man of his ability could ultimately bring himself to believe in his own "discoveries," and how his followers could have swallowed his doctrinal globules have always seemed to us amongst the most wonderful of psychological problems. To us the mere statement of Hahnemann's opinions on this subject appears sufficient refutation. He treated certain cerebral symptoms occurring in scarlet fever with tincture of opium in doses of 1-250,500th of a drop. This was in 1801, at the beginning of his "discoveries." Subsequently he reduced his preparation of drugs to a system. He "succussed" one part of a drug with 99 parts of sugar of milk or alcohol; this was the first "potency." One part of this triturated with 99 parts of the vehicle gave the second potency; and so on to the third and fourth. He had "discovered" that such preparations "influenced favourably the curative process." We are not surprised to learn that, until he became accustomed to it, his discovery "astounded" himself. He even calls it incredible. "In the first

years of his discovery he dwelt emphatically on the *weight* of the drug contained in his preparations, and recounted to the astonished world the results obtained by a millionth, billionth, &c., part of a grain of medicine." Has not the Creator, says Hahnemann himself, "bestowed upon us means and knowledge whereby we may diminish the more and most powerful substances into small and the very smallest doses and administer them in the tenth of a grain, the more powerful in the hundredth, the thousandth of a grain, the most powerful in the millionth, billionth—aye, even the trillionth, quadrillionth, quinquillionth of a grain?" Afterwards he "discovered" that the diminution of the action of the drug did not diminish with the quantity; then, that "with the above-mentioned mode of preparation the efficacy of many drugs, instead of diminishing, increased; that medicines so prepared gave results which could not be obtained with the crude substances." Then "the astounding fact became evident that medicines could be so diluted that neither physics nor chemistry could discover any medicinal matter in them, and yet they possessed great healing power."

"This," says Dr. Ameke, "is Hahnemann's greatest discovery—one of the most momentous discoveries ever brought to light by human research. By this discovery alone he became one of the greatest benefactors of the human species; it must inevitably work a complete revolution in the science of therapeutics, and will make its way for the weal of suffering humanity in spite of the keen opposition of university faculties and their unreflecting followers. No doubt in time the possibility of the action of such medicinal preparations will be explained by natural science" (p. 131).

It has become a matter of grave doubt whether the modern professing disciples of Hahnemann accept *ex animo* this part of the master's teaching. If they do, we wonder; if they do not, and yet allow the world to think they do, we wonder too.

Though weighted with this ponderous encumbrance, homœopathy still keeps afloat, its precarious buoyancy due to what was sound in Hahnemann's teaching, to the freedom from nauseousness of its medication, and to the ease with which old women and country clergymen can acquire (to their complete satisfaction), with the aid of a Manual and a case of globules, power of universal healing. The following is Dr. Dudgeon's account of the present state of homœopathy:—

"Germany, including Austria-Hungary and Switzerland, has upwards of 400 practitioners, four or five hospitals, and four journals. In Great

Britain there are upwards of 250 avowed, and a large but unknown and unavowed number of practitioners, three hospitals, and two monthly periodicals. In France there are more than 150 practitioners, two hospitals, and three monthly periodicals. In Russia there are about 100 practitioners and one or two periodicals. In Belgium there are twenty-seven practitioners and one monthly periodical. Italy has only forty-one practitioners and one monthly periodical. Spain has a large number of practitioners, two hospitals, and three or four monthly periodicals. In the United States of North America there are between 7,000 and 8,000 practitioners, fifty-four hospitals, several State-supported lunatic asylums, upwards of 100 societies (some of them numbering many hundreds of members), twenty periodicals, besides nine annual transactions of societies, and five annual reports of hospitals. The neighbouring British province of Canada has a considerable number of practitioners. Mexico has a good many, and a monthly periodical. In South America most of the States are well provided with practitioners; and several of them—as La Plata, Monte Video, Colombia—have homœopathic periodicals. Australia, New Zealand, Hindostan, and China, are all provided with homœopathic practitioners; indeed, there is scarcely a corner of the world where the disciples of Hahnemann have not penetrated” (p. 277).

The good points of Hahnemann’s teaching were the importance of honest pharmacy, the experimental method of ascertaining the effects of drugs, the necessity for careful regulation of diet, and the substitution of tasteless, or even tasty, medicines for the nauseous drenches in vogue with the physicians of his day. Further, he led the rising reaction against immoderate blood-letting—a reaction which has been only too successful, having resulted in the complete relinquishment of a valuable therapeutic aid. We shall conclude our remarks with some extracts, illustrative of what blood-letting was everywhere in those good old days, and still is, in regions in which Sangrado’s school is in the ascendant.

In 1819, Dr. J. R. Bischoff, senior physician to the General Hospital at Prague, in a controversial tract directed against homœopathy, maintained that “no harm had ever arisen from a right employment of bleeding, but great good had been done by it.” Accordingly, he himself had “bled two women of eighty-seven and ninety-seven years respectively with favourable results. He, in conjunction with a friend, bled a strong man twelve times in three days and a half, taking a pound of blood each time; and after the twelfth time profuse bleeding from the nose ensued twice, and the blood still showed a marked inflammatory coat. Nature gave thus the most convincing proof that not an ounce too much blood had

been taken. In six weeks the patient was entirely restored." In 1835 the Emperor Francis, 67 years of age, was attacked by "an inflammatory fever," and bled, of course. The fever subsided and recrudesced; and he was bled again. The symptoms became more urgent, and his physicians gave him up, but called in, for consultation, "three archiducal physicians in ordinary," who approved the previous treatment and declared that all was wanted for recovery was a profuse perspiration. "To bring this about they bled him twice more, after which the fever increased, the strength was proportionately diminished, the breathing became difficult, and within twenty-four hours the action of the heart stopped." There is something extremely pathetic in the account of the Emperor's dismissal of his doctors after their too-successful efforts to bleed him within an inch of his life. "He gave each of them his hand, thanked them for their exertions, and assured them of his love and favour, adding generously that he knew how much they loved him, and that they had done and would do all they could to save his life." After *post-mortem* examination the doctors reported that "the medical treatment was the only correct one, but the frequently-repeated bleedings had not been sufficient to restrain within limits the increasing inflammation, and a more energetic treatment was precluded by the general condition of the patient, and would have increased the danger of causing instantaneous death."

The story of Cavour's bloody death will bear repetition, as a "modern instance;" and wish it we shall bring our notice of Dr. Ameke's work to a close:—

"After a stormy sitting of Parliament on 29th May, 1861, in Turin, Cavour was seized with slight febrile rigor, to which, in the following night, 'violent pains in the bowels' and vomiting were added. Blood was drawn, 'which relieved the patient.' On the following morning, the 30th May, he was bled a second time, and again in the evening of the same day, at five o'clock, a third time. . . . Violent fever succeeded, the patient was 'very weak and suffering.' He passed a good night! Friday, the 31st, the fever disappeared, so that Cavour was able to hold a Council, which assembled round his bedside for two hours. In the evening he became very feverish. Quinine did no good. On the 1st June he was again bled twice; a quiet night followed. On the following day, June 2nd, he was pale and weak; his left hand and forearm cold as marble. . . . On attempting to leave his bed the wound in the vein re-opened, and the profuse bleeding could not be stopped until a surgeon was called in; some hours later violent fever, shortness



of breath, confusion of ideas. The night was very bad, and the next morning his excitement increased, his breathing became shorter, and severe thirst set in. . . . Cavour begged that a vein might be opened; this alone, he thought, could save him. The physician was quickly summoned; he consented, and a surgeon was sent for who made a new incision, but no blood flowed; by pressing the vein he succeeded in drawing off two or three ounces of thick blood. The incisions of the veins made on the first day were not healed. The consulting physicians prescribed a solution of sulphate of quinine. Cavour begged that it might be administered in the form of a pill, because he knew that the taste of the quinine would cause him to vomit. The doctor refused; he thought a solution better. He took the medicine with great repugnance; vomiting followed, and was renewed each time he attempted to take the drug, which he would only do at the persuasion of the friends who surrounded him. In the following night high fever and delirium; ice compresses on the head, and mustard plasters on the legs. The next night he was very bad again. Next morning cupping-glasses were applied to the nape, and again blisters on the legs. But the blisters would not rise, and the painful application of the cupping-glasses was not felt by the patient. Victor Emmanuel, who visited his Minister just before his death, proposed to the doctors to open a vein in his neck. The doctors promised to take the proposal into consideration, but death prevented them. Cavour died suffering from unquenchable thirst" (p. 259).

*The Principles and Practice of Medicine.* By RUSTOMJEE NASER-WANJEE KHORY, M.D., Brux.; M.R.C.P.L.; Fellow and Licentiate in Medicine of the University of Bombay; sometime Lecturer on Midwifery, Gujerati Class, Grant Medical College; Fellow of the Royal Medical and Chirurgical and Obstetrical Societies of London; Member of the Corporation of the City of Bombay. Second Edition, revised and much enlarged. Two Vols. London: H. K. Lewis. 1885. Pp. 1,297.

HAPPY is the people which has no history; and happy ought to be the author of whose work, as of this, there is little or nothing to be said by a reviewer. We find no fault with Dr. Khory's book. We only wonder why it should have been written. Systematic text-books of medicine abound, and we should not have thought that there was room for a new one, which, carefully and industriously compiled as it is, makes no pretence to originality, and offers scarcely anything new to the profession. Teachers in the

Indian medical schools feel keenly the want of text-books written for and suitable to India, but this is not one. It is not specially adapted to the Indian student, and specially Indian diseases are not discussed at greater relative length than others which are rare or unknown in the eastern inter-tropical countries. We must admit, however, that the fact that this is a second edition—exhibiting the usual tendency to hypertrophy of second editions of medical text-books—seems to indicate that Dr. Khory's work was demanded by somebody. The former edition, published in 1879, was a "Digest of the Principles and Practice of Medicine," and was, we are informed, so favourably received as to induce the author to prepare the present work. We fear that we ourselves should have preferred the infant "Digest" to the adult "Principles and Practice," partly because it was shorter, and partly because (if we rightly understand an obscure sentence in the preface) it contained some account of the medical system of Susruta, the Indian Hippocrates, or Hippocrates in Indian dress, and a letter on the same subject from some person oddly described as a "Fellow of the College." Should the author fulfil his intention of publishing a work on Indian *Materia Medica*, we hope that he will have become aware of the existence of Dr. Moodeen Sheriff's great work, the "Supplement to the Pharmacopœia of India," published at Madras in 1869, to which Dr. Khory makes not the slightest reference.

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*Lectures on the Diagnosis of Diseases of the Brain.* By W. R. GOWERS, M.D., F.R.C.P.; Assistant-Professor of Clinical Medicine in University College; Physician to University College Hospital and to the National Hospital for the Paralysed and Epileptic. London: J. & A. Churchill. 1885. 8vo. Pp. 246.

IN the number of this Journal for July, 1880 (Vol. LXX., No. 103, Third Series), we noticed in terms of unqualified praise Dr. Gowers' work on "The Diagnosis of Diseases of the Spinal Cord," which had then recently appeared. The closing words of our review of that most valuable contribution to the literature of diseases of the nervous system apply equally to the book it is now our privilege to bring under the notice of our readers:—"This volume will prove, we have no doubt, a most important stepping-stone to that perfect knowledge of the pathology of the [brain] which we hope one day to possess. It is full of information,

expressed remarkably clearly and concisely, and we heartily recommend it to our readers, and not less to the general practitioner than to the specialist."

Similar in scope and method to his former work, but of much larger size (246 pages compared with only 80), the present volume contains eighteen lectures which were delivered at University College Hospital. Of these lectures the first three are devoted to the medical anatomy of the brain, including a succinct account (pages 37-43) of its blood-vessels and the effects produced by various lesions in them.

Lectures IV. to XII. inclusive are on the symptoms of brain disease, embracing the motor symptoms (hemiplegia, convulsions, &c.), the sensory symptoms, cranial nerve symptoms, mental symptoms, affections of speech, headache, vertigo, vomiting, temperature, the pulse and respiration, the urine in cerebral disease, affections of the sphincter, and ophthalmoscopic changes. As an illustration of Dr. Gower's style we are tempted to quote his remarks on vomiting as a symptom of cerebral disease:—

"Vomiting is occasionally a most important symptom of cerebral disease, but it is a symptom that derives its significance chiefly from its associations. It has, indeed, certain characteristics, but these, which are chiefly negative, are common to the vomiting of functional nervous disorder and of organic disease. The act of vomiting is reflex, effected through a centre in the medulla, probably part of the vagus centre. It may be produced by an undue impression on the nerve-endings in the stomach—an impression that is felt as pain, discomfort, or nausea. Such is the vomiting of gastric origin, as that of ulcer of the stomach, or of simple indigestion. But vomiting may also occur from exalted irritability of the centre; and, in this condition, stimulation of the nerves of the stomach by the mere presence of food may be sufficient to excite the irritable centre. Such is the vomiting of hysteria, in which food is rejected as soon as it enters the stomach; and such is the vomiting of cerebral disease. The increased irritability of the centre may be so great that vomiting may occur without any peripheral irritation, when the stomach is empty.

"Nausea may or may not attend this increased reflex excitability. In hysterical vomiting it is generally absent, and it is often absent in the vomiting of organic cerebral disease. We know very little about nausea. It is an effect on the sensorium of the nervous irritation that excites vomiting, but is, as it were, a collateral disturbance, and is not due simply to the action of the reflex centre, since vomiting may occur without it. It is referred as much to the fauces as to the stomach, and

is prominent when vomiting is excited by tickling the fauces, and when the excitability of the centre is augmented by some other peripheral irritation, as that of the uterus. Hence, while its absence suggests, its presence is compatible with, a central cause. Organic disease in any part of the brain will cause vomiting, and it is therefore probable that the higher central relations of the pneumogastric nerve are very extensive—a fact that is also suggested by the varied functional disturbance of the cerebral centres that may result from gastric disturbance, and by the singular readiness with which vomiting may be excited by olfactory impressions and by some psychical influences. You may perhaps know Weir Mitchell's story of the man who was so much disgusted by his wife's vomiting during her first pregnancy, that he vomited too; and ever after, when his wife became pregnant, he became sick. Although vomiting may be produced by disease in any situation, it is caused with especial readiness by disease of the pons and medulla, and most readily of all by disease of the cerebellum. It has been thought that this is due simply to pressure on the subjacent medulla, but it is probable that a functional mechanism is concerned in the effect. Of all the pathological relations of vomiting, the most remarkable is its relation to vertigo. Of this relation I have already spoken, and will only further remark that this association, coupled with the curious disturbance of equilibrium that results from disease of the cerebellum, suggests that the vomiting of cerebellar disease is due to an intimate relation between the gastric centre and this part of the brain.

"Cerebral vomiting results chiefly from irritating disease. It is rarely due to a stationary lesion. Meningitis, tumour, abscess, cause it; acute lesions only at their onset, or during the period of secondary irritation. There are two facts regarding cerebral vomiting that I would impress upon you. The diseases that cause vomiting usually cause also pain in the head, and the vomiting often attends paroxysms of pain, and adds much to their significance. They give a similar significance to pain that is slight, and would not, in itself, suggest organic disease. The second fact is that brain disease, which exalts the excitability of the gastric centre, does so gradually, and the vomiting may be at first excited by a peripheral cause—a cause that might not be effective were it not for the exalted excitability of the centre. In such cases the exciting cause may be accepted too readily as an adequate explanation. This caution is especially necessary in the case of children. I have more than once known the vomiting of meningitis or of cerebral tumour to be, at first, excited by some injudicious food, and to be therefore thought to be of no importance, until graver symptoms gave significance to it. Remember that indigestion, sufficient, by itself, to make children vomit, usually makes them ill; and if a child vomits food without seeming otherwise indisposed, do not at once assume that there is nothing more

the matter with it than indigestion, but watch it carefully." (Pages 150-152).

The remaining lectures—six in number—deal with the question of diagnosis of the seat of cerebral disease. We cannot see, however, that there was any need for the author to speak of them in his preface in the half-apologetic way he does, for they are probably the most interesting and useful chapters in the book. "Localisation" receives full attention in the thirteenth and fourteenth lectures, while "Pathological Diagnosis" occupies the four succeeding ones.

Speaking of hysteria\* Dr. Gowers writes:—"There are few organic diseases of the brain that the great mimetic neurosis may not simulate;" and he then proceeds to portray the differential diagnosis between "the often-perplexing symptoms of hysteria" and organic cerebral disease. But we must refer our readers to the work itself for this masterly analysis, and for the mine of information contained in every page of Dr. Gowers' modestly-written and instructive lectures.

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*Transactions of the Pathological Society of London.* Vol. XXXVI.  
1885. Pp. 575.

THIS volume quite equals the best of its predecessors in quantity and quality of material. The plates (20) and woodcuts (26) are of their usual excellence. Taking the volume, and opening it at hazard, we dropped on a communication on fractures of the hyoid bone in relation to fractures of the thyroid and cricoid cartilages, which develops a subject not very generally treated of. There is also one on fractures of the first rib alone, tending to show the means by which this rib alone is fractured, and that it is not such a rare fracture as the writings of surgical authors would lead one to suppose. There is a case of carcinoma of stomach in a child aged thirteen, which appears to be the first (established by microscopic examination) hitherto recorded in England at so early an age. A work on "Precocious Cancer" was lately reviewed in the pages of this Journal. There is an interesting case of gonorrhœal pyæmia, with thrombosed vaginal veins and plugging of the primary branches of the pulmonary artery with secondary emboli. The patient, a girl aged nineteen, was walking across the ward, when she fell down, and died almost instantaneously. The volume con-

cludes with a highly interesting report on "Diseases of the Reproductive Organs in Animals."

The present volume will be recognised by all workers as a mine of pathological treasure.

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*Voice Use and Stimulants.* By LENNOX BROWNE, F.R.C.S., Ed.; Senior Surgeon to the Central London Throat and Ear Hospital. London: Sampson Low, Marston, Searle & Rivington. 1885. Pp. 147.

THIS book is "a considerable extension" of a lecture delivered by the author before the Society for the Study and Cure of Inebriety. The subject treated of is one of considerable importance not only to singers, but to clergymen, lawyers, actors, and to all those whose professional duties require much use of the voice; and the author assures us he would not have entered into the subject had not a matured experience convinced him "that the career, and not seldom the life, of a large number of professional voice users is cut short prematurely by unwise, though not always excessive, indulgence in alcohol." Though the greater portion of the book deals with the influence of stimulants on the throat, in the latter portion the author discusses the question as to the effect of tobacco on voice use, a question "incidental to the question of alcohol, and by an almost natural sequence."

The alcoholic question has hitherto been too much generalised. Insufficient attention has been paid "to the logic of carefully ascertained and duly balanced facts." "It might be well for representatives of each divisional field of human disease to apply themselves to investigate the influence of alcoholic stimulants on those organs with which, in the daily exercise of their profession, they are more immediately occupied." The author deals with the subject in the narrower domain in which he is an expert specialist. First he refers to the literature of the subject, and, as we should expect, finds it very scanty. "In drunkards," Lauder Brunton says, "there is a great tendency to chronic catarrh of the respiratory passages; the back of the throat is often red and congested. This congestion extends down to the larynx, giving rise to hoarseness and expectoration of mucus." In the "Cyclopædia of Practical Medicine," published in 1834, attention was first called to alcoholism as a cause of phthisis; and to this form of the disease Dr. Richardson, in 1861, gave the name of *alcoholic phthisis*, or the

*consumption of drunkards.* The class of individuals who are thus affected are not those who are in the ordinary sense drunkards, "they may never have been intoxicated in their lives, but they partake freely of any and every alcoholic drink that comes in their way."

The subject which chiefly occupies the author is, however, not that of the drunkard, where all are pretty well agreed, but the more controversial question as to the effects of stimulants on the voice when taken in more moderate quantities. "The voice is the hygrometer of sobriety," and this, evidently, is the opinion of the author. The effects of intemperance are well-marked and characteristic. *Aphonic* and *psellismus potatorum* are diseases mentioned in Virchow's Handbook; chronic inflammation of the larynx in toppers giving rise to hoarseness is styled "*raucedo potatorum*" by Tobold. Gottstein notices a peculiar thickening of the epiglottis, and Crichton Browne observes that "in *alcoholismus chronicus* there is undoubtedly hoarseness not due to any swelling of the mucous membrane, but to paralysis dependent upon brain wasting." The author enumerates certain conditions which he considers will differentiate the "drinker's throat." Sensitiveness of the mucous membrane, venous congestion, frequent disposition to local inflammation, especially of the tonsils, general infiltration of the submucous tissue, relaxation of the uvula accompanied by paresis, granulations, and a similar condition of varix at the base of the tongue. In the larynx he observes a constant disposition to congestion of the mucous membrane, and to more or less acute inflammation; later on chronic inflammation and thickening of the tissues, especially of the epiglottis, and in a few advanced cases a nodular condition of the vocal cords. These last structures act tremulously and uncertainly.

All these symptoms may be present, as Cohen observes, "in a marked degree in individuals altogether unaccustomed to the habitual use of stimulants."

The evils of the alcoholic habit are well described by the author when he says—"In addition to recurrent hoarseness, I have over and over again observed a general uncertainty in intonation, the tendency being to sing flat, a gradual loss of high notes, diminished resonance, and a want of precision in both verbal and vocal utterance in singers who could not be accused of alcoholic indulgence to anything like intoxication, but who have certainly drunk to excess in relation to work, and unwisely in relation to the period of indul

gence. In most of such subjects chronic dryness of the throat is complained of, which, although the cause (effect?) of a bad habit, is often made an excuse for its continuance. The taking 'a hair of the (alcoholic) dog that bites them' is, however, an especially fatal practice in the case of voice users."

The greater portion of the book is occupied by an analysis of the opinions held on this subject by 380 male vocalists who replied to a circular letter addressed to them by the author. The questions asked are thus given:—

- "1. Are you in the habit of taking alcoholic stimulants?
- "2. If so, in what form; that is, as ale, stout, wine or spirits?
- "3. What is your general habit in this respect? I mean, is the stimulant taken—at meals, between meals, at the end of the day, or at pleasure and opportunity?
- "4. Do you take any stimulant either immediately before or during use of the voice, as an aid to its exercise?
- "5. Are you in the habit of tobacco smoking?
- "6. If so, do you smoke pipe, cigar, or cigarette?
- "7. Have you any objection to publication of your name?"

Only ten objected to the publication of their names. It is, in our opinion, a matter of doubt whether much importance can be, or ought to be attached to statistics of this nature, and the author himself in several places seems to hold the same view, as several replies which do not agree altogether with his conclusions have been largely discounted.

Some of the results of this investigation are, however, interesting. Grouping together the *habitual* with the *occasional* drinkers, and again, those who abstain with those who indulge *rarely*, "we find the numbers of the former 254, and of the latter 126"—that is, as nearly as possible two to one. Again, "one-third of the whole number take ale or stout—more frequently the latter—as their sole beverage; and upwards of three-fourths take malt in combination with either wine or spirits, or both. "Of the wine-drinkers, the majority specify claret as the kind preferred for ordinary use."

"Three-fourths of our whole professional singers" do not believe in the power of alcohol to directly aid voice use; or, subtracting the 101 abstainers, we find as nearly as possible two-thirds of those who take alcohol with their meals abstaining from it in connection with work."

In summing up the results of his investigations the author concludes that, while alcohol is capable of directly producing injury to



the vocal organs, it is of little value as a direct aid to work, and if required is, as a rule, permissible only when work is concluded.

About 30 pages deal with the subject of tobacco in relation to voice use, and the same line is taken up as in dealing with the alcohol question. First the literature, then the medical view of the evil effects of excessive smoking, and lastly, the personal testimony, are taken *seriatim*. The author thus expresses his own view:—If smoking be accompanied by much expectoration, it should be discontinued, as an over-stimulation of the salivary glands will lead to general dyspepsia, and later to local dryness. The evidence of my correspondents on the fact that smoking does lead to this symptom is very strong. In all cases the singer must be guided by his own individual experience, and should practice great moderation in the habit. Before dismissing the subject of tobacco-smoking the author raises his voice against a form of entertainment which is undoubtedly injurious to the voice user—namely, smoking concerts. If such entertainments must be held, they should, as far as performers are concerned, be instrumental and not vocal.

Finally, we may say that this book—short, well written, and handsomely brought out—is full of suggestions for those whose voices are to bear the brunt of the battle of life, and we doubt whether such, after carefully perusing it, will not come to the conclusion that in their professional interests they had better become abstainers both from alcohol and tobacco.

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*The Blot upon the Brain : Studies in History and Psychology.* By WM. W. IRELAND, M.D. Edin. Edinburgh: Bell & Bradfute. 1885. Pp. 374.

THIS is a book which may be described in the words the author uses (p. 308) in reference to Dr. Wigan's book "On the Duality of the Mind"—"a very readable, though somewhat desultory work."

We think the latter portion of the title—viz., "Studies in History and Psychology"—would have been more appropriate; we cannot see the application of the former part. The book consists of thirteen papers, the first six of which are chiefly historical, the remainder chiefly psychological. Some of the papers have already appeared in the *Journal of Mental Science* and in *Brain*. The author, who was formerly in H. M. Indian army, has produced several other works on various subjects. We doubt that this book will have any more fixed or definite place in medical literature

than have articles on cognate subjects which, from time to time, appear in the *Contemporary Review* or *Nineteenth Century*. There is in it much physiology and some psychology, which all well-informed medical men are familiar with, as well as many quotations from foreign writers; but we do not feel that our insight into any of the great questions discussed in the psychological part has been increased or illuminated.

The Historical part, especially Papers 2 to 6, is most interesting, and will well repay careful perusal. These papers indicate careful and minute research, or else access to mines of information not generally accessible.

The book is well put out, and has a sufficiently good index.

### RECENT WORKS ON SURGERY IN INDIA.

*Operative Surgery in the Calcutta Medical College Hospital.* By KENNETH M'LEOD, A.M., M.D. London: J. & A. Churchill, 11 New Burlington-street.

*Litholapaxy: One Hundred and Eleven Cases.* By P. J. FREYER, M.A., M.D. Calcutta: Thacker, Spink, & Co. London: W. Thacker, 87 Newgate-street.

*Forty-two Cases of Litholapaxy: a Further Series.* By P. J. FREYER, M.A., M.D. Reprinted from *Indian Medical Gazette* of April, 1885.

WE have grouped these publications as records of what important surgical work is being done in our Indian Empire. Both authors are in the Indian Medical Service, and the history which they here give of their work shows how fully they have taken advantage of the great field which they have chosen for their enterprise.

The first on the list is a considerable volume. In it the author relates what cases came under his care during the five years of his surgeoncy to the hospital; what he did with them, and what were the results. The author adopts an unusual but a very honest method. He rightly points out that statistics of operations are too often apt to be fallacious from the circumstance that they are collected from different authors, and that these authors do not, as we know, always report their unsuccessful cases. A record, therefore, that presents, as this does, all the cases treated, and gives a short account of each, supplies us with the most reliable form of

results. It is an example that might well be followed by those who publish hospital reports.

We have a few interesting notes on the physique of the Lower Bengalee, which utterly dispel some of the illusions which many home surgeons have regarding them. We have often heard it asserted that the East Indian was the most perfect being physically on whom a grave operation could be performed. His temperate habits, the character of the food, were supposed to account for the success which attended the operations for stone done by the untaught native. But Professor M'Leod gives us this picture of the class from whom his patients came in Calcutta:—

“The native of Lower Bengal is physically of feeble type. His temperament is of the fibrous or bilious type, with a strong nervous element in the higher classes, and a lymphatic tendency in the lower. The races of Upper India exhibit arthritic features, but these are entirely absent in the organisation of the Bengalees. Their circumstances and habits of life are not favourable to the development of a vigorous physique, to good health, longevity, or strong vital resistance. They live for the most part in crowded and filthy villages, they lead lives of toil and privation, they drink foul water, subsist on a sparing dietary composed mostly of rice, vegetables, oil, and spices, to which vetches, fish, and very rarely flesh and milk, are added when they are able to afford it. They live in the midst of malaria, and are constantly harassed by fever and its complications and sequelæ. Occasionally epidemics of malarious fever prostrate and decimate large and populous districts. Many of them exhibit chronic anæmia, enlargement of spleen and liver, diarrhœa and dysentery, set up by repeated attacks of malarious fever. Syphilis is very rife among them, and mercury is too often rashly and excessively administered for its cure. They are not, as a rule, addicted to spirits, but in the larger villages and towns, more especially in Calcutta and its suburbs, alcoholic excess is by no means unfrequent, and is becoming more so. Many of them consume opium and hemp to excess, and the use of tobacco and *pan* is universal. The better classes are apt to indulge freely in sweetmeats, and *ghae* (clarified butter), and excessive fatness and diabetes are very common among them. Add to this, infant marriage and often unrestrained sexual debauchery, and the picture is by no means a bright or promising one as far as disease or operation is concerned.

“A belief exists that native Indians are remarkably tolerant of severe operations. This is true of up-country natives, but as far as the inhabitants of Calcutta and its neighbourhood are concerned, I am entirely in accord with Sir Joseph Fayrer when he writes (‘Clinical Surgery in India,’ page 30): ‘So far from being favourable subjects for surgical

operations, I regard them as quite the reverse, and feel assured that to the surgeon who has had the opportunity of treating serious wounds or operations in the rustic native, the difference must be as remarkable as it is discouraging.'

"It must also be added that natives do not, as a rule, resort to hospitals for treatment until their disease has assumed serious and often irremediable intensity. They do not exhibit either pluck or hope when subjected to operation, their mental attitude mostly being a callous fatalism or a calm despair. As regards other classes of Asiatics—Jews, Armenians, Parsees, &c.—they present a better physique than the natives of Bengal; but in constitution and health they are undoubtedly inferior to European races."

And Dr. Freyer, working in the North-west Provinces, also observes:—

"I have referred to the erroneous impression which prevails that the mortality from lithotomy in natives is less than in the case of Europeans. There is also an impression prevalent to the effect that natives of India have no fear of the surgical knife. In fact, from the way some people talk and write it might be almost inferred that a native submits to a surgical operation as a kind of harmless diversion. This impression is altogether erroneous. A native of India will not, as a rule, submit to a surgical operation till all other modes of treatment fail, and he is driven to it through extreme pain, inconvenience, or danger to life. And it is for this reason that such large calculi are met with in this country, and that patients suffering from cancer and other diseases present themselves in hospital at a stage when surgical interference is useless."

We see, then, that instead of being favourable subjects for surgical operations, the East Indians are quite the reverse. Indeed, the rapidity with which some of these people succumb to slight injuries, accidentally received, is a matter of common observation among Anglo-Indians.

Dr. M'Leod mentions that while acute diseases of the articulations are very common, chronic disease of joints of a strumous description and origin is extremely rare. *Morbus coxæ* is seldom, if ever, met with except among Eurasians and Europeans. These facts have also been noted by Mr. C. Macnamara, of London, who had a long Indian experience. The author has never seen a case of rickets in a native, and *mollities ossium* is very rare.

129 cases of elephantiasis of the scrotum were under treatment, and of these 23 died. The method of operation is described at length. It is often one of great magnitude—always one of diffi-

culty; and having regard to these facts, and the complications of hernia, hydrocele, and fistula, the results would appear to be sufficiently favourable: 9 died from tetanus and 6 from septicæmia, 1 from gangrene, 4 from exhaustion, and 3 from "other causes."

The chapter on operations for hernia shows 72 operations, being 8 in cases complicated with elephantiasis (4 deaths), 18 for strangulation (9 deaths), 17 for radical cure by Wood's method (1 death), 29 by antiseptic ligature and removal of sac (2 deaths). The total mortality is 22·5—rather large, especially in cases of strangulation; but here the surgeon's statistics suffer from the fact that in India, as at home, the patient too often puts off the search for relief until hopeless damage has been done to the gut. In many of these cases the sac was ligatured and removed for the purpose of radical cure.

In 17 cases Wood's operation was done, with one fatal result. But this was in no sense due to the method. A portion of gut had remained in the scrotum and become strangulated, and while an operation for its relief was being done the patient died of asphyxia. It was owing to occasional failures of Wood's operation, and Dr. McLeod adopted the open or dissection method. Here the mortality was 6·9; and in two cases he mentions gangrene of the testicle resulting from pressure of the narrowed ring upon the cord. His plan of treatment is given with much detail.

We learn that malignant tumours are very common among the natives of Bengal—a conclusion confirmed by private practice, and one which is entirely at variance with the teaching of some textbooks. In two cases the larynx was removed for epithelioma; one patient lived for 5½ months, the other died of secondary hæmorrhage on the fifth day—results which go to support the opinion that such operative procedures are brilliant only for the surgeon, and are not of much advantage to the sufferer.

Chapter VIII. gives a record of 61 amputations, with 16 deaths; 38 were for injury (9 deaths), and 23 for disease (7 deaths)—a percentage of 31·9. The thigh amputations, of which there were 14, show a mortality of 57·1 per cent. Fayrer has already remarked upon the great fatality of thigh amputations in Calcutta and in India generally. He refers it to the unsatisfactory condition of the patients, and to the great tendency in them to suppuration and various forms of sepsis. The tables now published show a marked improvement, and are attributed to better hygiene and the use of antiseptic dressings. There are difficulties, however, in

the proper carrying out of details in regard to the latter. "Careless and heedless assistants and unintelligent and reckless patients, and difficulties in obtaining the necessary appliances, render failures more frequent than they ought to be." But Dr. McLeod is satisfied of the safety which the system affords, and he is determined to carry it out more strictly on his return to Calcutta, "feeling assured," he says, "that if I succeed in so doing better than I have done, my results will also be better than those exhibited in this record."

There are many interesting cases in this work, to which we have not space to refer. But we hope it will be read by home surgeons. It is full of suggestiveness. The model is excellent and worthy of imitation. There is altogether a great deal to be learnt from it, as a record of clinical work, and it bears from cover to cover the impress of the truth, and the whole truth.

We now turn to Dr. Freyer's pamphlets. We are glad to recognise in the author a young Irishman—a graduate of the Queen's University, who has already made himself distinguished in the Indian Medical Service. They are altogether devoted to the subject of stone in the bladder, and to the operative methods of dealing with that serious condition. As is well known, the natives of India are peculiarly liable to stone, and this is shown by the fact that in the year 1882, 2,730 cases were operated on in Indian hospitals, not to mention the large number which submitted to the native cutters.

Dr. Freyer has latterly devoted his attention almost exclusively to litholapaxy, and with results which entirely justify his predilection. There can be no doubt that Bigelow has revolutionised procedure in this particular department, if we exclude children, in whom the undeveloped condition of the urinary and genital organs render it dangerous to introduce a sufficiently large lithotrite and evacuator into the bladder. Sir Henry Thompson, in his work on "Diseases of the Urinary Organs" (1879), speaking of Bigelow's method, observes:—"Of this proposal I feel compelled to say that although the results may often be successful, it is to be feared that they must sometimes be disastrous." The instruments were likened to the "terrible engines of Heurtaloup," and described as "enormous and unwieldy." It was this strongly-expressed opinion that forced Dr. Freyer to adhere to cutting only in his early cases. In 182 lithotomies he had 9 deaths. But these occurred altogether

amongst adult males, of whom there were 50 (18 per cent. mortality), while in 132 cases of male children under the age of puberty there was not a single death.

The change in Thompson's views as to litholapaxy (or lithotritry at one sitting, as he prefers to call it) turned Dr. Freyer's attention again to that operation, and he has since performed it practically in all cases except children. The results which he now publishes are very remarkable. In 115 adult males treated by litholapaxy there were 4 deaths, as against 9 deaths in 50 treated by lithotomy. Uric acid calculi over 3 ounces in weight have been entirely removed at one sitting. The patients ranged in age from over 90 to puberty, giving an average of  $46\frac{1}{4}$  years. The cases which succumbed would probably have died under any operation, the kidneys being found extensively diseased. The time occupied in the operation varied from 52 minutes to 1 minute, and the largest stone weighed 3 oz. 2 dr. The convalescence was particularly rapid, and the patients' average stay in hospital was  $8\frac{1}{4}$  days. The author is naturally thoroughly satisfied as to the superiority of crushing the stone in the great majority of cases; and he expresses his opinion thus:—"The surgeon who would give his patients suffering from stone the best prospect of recovery must practise litholapaxy."

A suggestion as to the detection of small calculi is valuable. In some cases it was impossible to find them with the sound, but on using the aspirator "a distinct click was heard during the exhaustion of the water, due to the calculus being carried with force against the eye of the canula by the outward stream."

We hope Dr. Freyer will give us further records of his work, of which he now offers so good a specimen. His example cannot fail to have a marked effect in India upon this class of surgery; and, as in the case of the book first referred to in this notice, we strongly commend the reading of his reports to surgeons in these countries.

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*Face and Foot Deformities.* By FREDERICK CHURCHILL, C.M.; Surgeon to the Victoria Hospital for Children. London: J. & A. Churchill, 11 New Burlington-street. 1885. 8vo. Pp. 195

EXCELLENT printing, fine paper, some chromographs and monotones, are the important characteristics of this book. But we are not quite sure what purpose it is intended to serve. It is certainly not an exhaustive treatise; it is certainly not fit to "cram" a student for his

examination; and we do not see what good it can do its author's reputation. We are caught at once by the title, and we ask ourselves, why "face and foot?" unless it be that there is a certain musical swing in the phrase. We find, oddly enough, that the same question presented itself to Mr. Churchill, and we have his problem thus put and solved: "It may be asked why I should associate face with foot, as these are the two extreme parts of the body? Chiefly because deformities of these members of the body being more manifest than deformities elsewhere, they constitute a greater hindrance to success in life. They not only deform but deface, by reason of their great disfigurement, the artistic proportions of those parts of the body upon which the eye of the observer or critic most loves to rest." This is mere poetry. A naked foot, except his own, is not often seen by the lay critic, and corns and crumpled toes are not very obvious through shoe leather. Surely, the "eye of the observer" more frequently examines "the artistic proportions" of a hand, and notes any departure from symmetry. Why not, therefore, "hand," also, unless our author feared it would spoil a rhythmic title?

We find that Mr. Churchill widens very much the usually accepted definition of "deformity," and brings under it all surface changes—"a deformity, in fact, is anything that is manifestly *ugly* or *crooked*." Now, we entirely object to this definition. It is not scientific; and it is not true. Let us see its effect when put in operation. It has enabled the author to drag into the chapters on deformities of the face most things from tetanus to a pimple—eye affections, skin diseases, fractures, dislocations, and constitutional conditions, such as hysteria, so far as they exhibit themselves in the face. Then, under the heading "foot," we travel from chilblains to compound dislocation of the ankle; including, on the way, various congenital and acquired conditions. Yet some of the most important abnormalities are omitted altogether. All this is most objectionable, and looks like an attempt to establish a new field of surgery for "special" cultivation. We protest against the suggestion that such a thing as a black eye is a "deformity," although it may be "ugly." To say that "deformity is anything that is manifestly ugly" is absolutely wrong, and most unjust to many highly cultivated and estimable human beings.

Building upon such a basis as this, we are not surprised that Mr. Churchill has not produced a very readable or instructive book. There is a bewildering little-of-most-things, with added apologies



that he has not given us more. We hope that he will not for the present, at least, carry out any design in that direction. He has some facility in writing, but he lacks method. He has failed because he has written up to a title. If he will only abandon this, and devote himself to a definite line, instead of meandering through medicine and surgery, he will, we have no doubt, produce a book which will be creditable to himself, and in some degree useful to his professional brethren. Meanwhile, we wish it were possible to create a well-paid board of censors to revise the manuscript of medical books before they are sent out to the profession. It would pay the hard-worked physician and surgeon even if he were subjected to a tax for the saving of his time, by escaping the perusal of one-half of the literature that is now furnished for him. Life is too short for the labour as it is at present.

*Clinical Lectures on Diseases of the Liver, Jaundice, and Abdominal Dropsy: including the Croonian Lectures on Functional Derangements of the Liver, delivered at the Royal College of Physicians in 1874.* By CHARLES MURCHISON, M.D., LL.D., F.R.S., &c. Third Edition. Edited by T. LAUDER BRUNTON, M.D., Sc.D., F.R.C.P., F.R.S. With Section on Tropical Diseases by SIR JOSEPH FAYRER, K.C.S.I., LL.D., M.D., F.R.C.P., F.R.S. With 43 illustrations on wood. London: Longmans, Green & Co. 1885. 8vo. Pp. 702.

PROBABLY in no language have two more magnificent and exhaustive monographs appeared than those by the late Dr. Charles Murchison on the Continued Fevers of Great Britain and on Diseases of the Liver. It was, therefore, fitting that a new edition of both treatises should be called for and supplied. The author's sudden and lamented death forbade that the masterhand which wrote the works should revise them more than once, and so it became necessary to select an editor in each case. To Dr. W. Cayley was entrusted the task of bringing out the third edition of the Treatise upon the Continued Fevers, and, as the result proved, no more judicious choice could have been made. With equal good taste and skill, Dr. Cayley succeeded in maintaining the individuality of the work, while he embodied in its pages all that was new and valuable in the modern literature of "Fever"—the book is still Murchison's splendid monograph.

And now, under almost precisely similar circumstances, a third

edition of the scarcely less celebrated "*Clinical Lectures on Diseases of the Liver*" has issued from the press, and it is our pleasing duty once more to congratulate the publishers, Messrs. Longmans, Green & Co., on their wise choice of an editor who has preserved intact the character of the book. In the preface Dr. T. Lauder Brunton says that "in preparing a new edition of this work," he "has tried to preserve its individuality unimpaired, by making such additions or corrections only as were rendered necessary by the advance of medical science since the appearance of the last edition. All additions and alterations have been enclosed within square brackets, so that they can be at once distinguished by the reader." We feel sure that the medical public will recognise with gratitude Dr. Lauder Brunton's self-denial and sound judgment in this matter—editorial qualities which, on the present occasion, have saved to another generation of medical readers this classical treatise.

One portion of the work, however, has undergone a more sweeping revision at the hands of Sir Joseph Fayrer, K.C.S.I., F.R.S.—namely, the section on tropical diseases of the liver—inflammation and abscess. On these topics Sir Joseph may well be assumed to speak with authority from his long and extensive Indian experiences, and we are therefore willing to condone the freer use of the pruning-knife and the practice of engrafting which has been in this instance adopted. At page 190 a series of paragraphs has been interpolated to show the relation of liver abscess to dysentery, but it is in the section on Treatment that the most extensive alterations and additions are met with. They have reference chiefly to the rules for operative interference, the mode of operating, the site of exploration and the time for opening an abscess of the liver. This last "should be as soon as the presence of pus is detected, and this may be effected by aspiration with the long delicate needle, in some cases before any physical sign of its presence can be detected." The writer adds:—"The introduction of the needle for exploration in suspicious cases is free from danger, and itself occasionally confers relief." (Page 213.)

The use of the words "never" and "always" is to be deprecated in clinical medicine, for here as in most other things there is no rule without an exception. Experience leads us to object to the dogmatic assertion on page 378—for which Dr. Murchison was primarily responsible—that the pain in biliary colic radiates "never downwards." This is true in general, but many cases occur in which the pain does radiate downwards—not in lead into the testicle,

as in renal colic, or into the lower limbs, but to the iliac fossæ and symphysis pubis. We draw attention to this *lâche*, because Dr. Murchison was usually so accurate in his descriptive phraseology.

At page 442 will be found an interpolated account (after Stadelmann) of the production of jaundice by a substance belonging to the aromatic series of hydrocarbons, to which the almost unpronounceable name "Toluylendiamine" has been given.

In two respects this edition is decidedly improved. First, by the substitution, on pages 2 and 3, of illustrations after Luschka, showing the relations of the liver to other viscera, for the very indifferent woodcuts in the former editions. And, secondly, by the introduction of heavy (Egyptian) type in the headings of the sections and paragraphs, and occasionally in the body of the paragraphs. In this way the reader's eye is caught, and his attention is arrested and fixed on the more important points in the description.

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*A Practical Treatise on Fractures and Dislocations.* By FRANK HASTINGS HAMILTON, late Professor of Surgery in Bellevue Medical College, New York. Seventh Edition. London: Smith, Elder & Co., 15 Waterloo-place. 1885.

It is about twenty-five years since the first edition of this great work appeared. The edition now issued is the seventh, and this fact alone is enough to testify to the excellence of it in all particulars. Books upon special subjects do not usually command extended sale; but this one is without a rival in any language. It is essentially a practical treatise, and it gathers within its covers almost everything valuable that has been written about fractures and dislocations. There are some omissions, of course, as is to be expected, but these are very few. The principles and methods of treatment are very fully given. Ample justice is done to inventors of appliances of all kinds; and the surgeon will find with the descriptions of these sufficient comment to guide him to his choice. The book is so well known that it does not require any lengthened review. We can only say that it is still unapproached as a treatise, and that it is a proof of the zeal and industry and great ability of its distinguished author.

## PART III.

### MEDICAL MISCELLANY.

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*Reports, Transactions, and Scientific Intelligence.*

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#### ACADEMY OF MEDICINE IN IRELAND.

President—ROBERT M'DONNELL, M.D., F.R.S.  
General Secretary—W. THOMSON, M.D.

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#### OBSTETRICAL SECTION.

President—T. MORE MADDEN, M.K.Q.C.P.  
Sectional Secretary—WILLIAM C. NEVILLE, M.D.

*Friday, November 27, 1885.*

The President in the Chair.

#### *Pathological Specimens.*

DR. KIDD showed a double foetal monster, aged about five months, which had been forwarded to him by Dr. Leeper, of Armagh. The specimen had been retained *in utero* for some time after its death, and upon delivery was already somewhat lacerated and decomposed. Decomposition had not been entirely checked by the spirit in which it had been kept, but the main features of the monstrosity were quite apparent. There were four upper and four lower limbs, and a single head. Two completely-formed bodies were united to one another along the line of the sternum and the anterior abdominal wall; and a single funis was attached to the lower part of the abdominal wall. It was therefore one of the most common forms of a double monster. For very many years the formation of such monsters had puzzled pathologists—some affirming that they originated in the partial cohesion of separate embryos, thus indicating duplicity tending towards unity; and others affirming, on the contrary, that they resulted from the double formation of individual parts in a single embryo, being thus the expression rather of unity tending towards duplicity. The researches of Dr. Allen Thomson had settled this question in favour of the latter hypothesis, which was now the theory almost universally believed to be the true one.

DR. J. A. BYRNE exhibited an ovarian tumour which he had removed about a week since from a patient in St. Vincent's Hospital. The tumour was chiefly composed of a large thick-walled cyst, with some more solid matter towards its base. The patient had recovered well without any bad symptoms, though a large quantity of the cystic fluid had escaped into the abdomen during the operation.

*Inaugural Address.*

The President of the Section, DR. T. MORE MADDEN, then delivered his opening address on "Recent Progress in Obstetric and Gynæcological Medicine." [It will be found in the Number of this Journal for January, 1886, p. 18.]

*Axis-traction in Instrumental Delivery, with Description of a Simplified Axis-traction Forceps.*

DR. NEVILLE read a paper on the above subject. [It will be found at p. 97.]

DR. MACAN thought Dr. Neville gave too much credit to Dr. Tarnier and too little to Hubert, Hermann, and others, who had been working at the same problem of axis-traction before him. He did not attribute the same importance as Dr. Neville did to the special construction of Tarnier's forceps allowing of rotation, because he believed that rotation could, and often did, occur within the blades of the forceps without their partaking in it. He had himself used Dr. Neville's forceps in several cases in the Rotunda Hospital and found it answer extremely well. The elimination of traction-rods he considered an immense improvement, and the ease with which the traction-apparatus could be applied, and also its portability, were distinct improvements. It would be the better of having some kind of indicator, which would show the exact direction in which to apply traction.

DR. ATTHILL said that the instrument devised by Dr. Neville appeared to him to be the best and simplest axis-traction forceps he had yet seen. He was not, however, an advocate of axis-traction forceps. He objected to the compression-screw, which seemed a necessary part of them all. It might cause such a pressure on the child's head that death would follow. He had used Tarnier's first model when he was Master of the Rotunda, but found it so complicated and difficult to apply when the head was above the brim that he had abandoned it.

The PRESIDENT said they were indebted to Dr. Neville for the very practical axis-traction forceps he had shown them. He doubted, however, whether axis-traction would long continue to be practised. Tarnier's instrument he considered entirely too complicated.

DR. NEVILLE, in reply, argued that, from a practical point of view, the principle of axis-traction had been amply vindicated by the fact that,

with few exceptions, it had been recommended and advised by the leading obstetric authorities at home and abroad. Among English authorities might be cited Dr. Barnes and Dr. Playfair. Axis-traction saved force by economising it, and the maternal tissues were saved from an injurious and needless pressure. The action of the fixation-screw was not, in his opinion, so injurious as Dr. Atthill thought it might be. Practically it had not been found to injure the child, and was, indeed, only intended to keep the blades from slipping by the opening of the handles during the application of traction. It should not be called a compression-screw, for if properly used it had nothing to say to compression. While fully recognising the great value of the old forceps, he would not like to commit himself to stating that it was incapable of improvement.

#### *Fibro-Myomatous Polypus.*

DR. KIRKPATRICK exhibited a fibro-myomatous polypus removed from a multiparous woman in Sir P. Dun's Hospital. The patient had been suffering from symptoms of a uterine tumour for over two years, and of late had frequently had alarming hæmorrhages. The tumour had several times protruded from the vagina, and been replaced by the patient herself. When about to remove the polypus he had found it separated by a very distinct pedicle from a completely inverted uterus. He removed it with an ecraseur, and some vessels were tied in the stump, which was afterwards seared with an actual cautery. He had not since had an opportunity of examining the parts. The tumour, examined microscopically by Dr. H. Bewley, proved to be most unusually vascular.

DR. MACAN showed a somewhat similar polypus removed from a patient under his care in the Rotunda Hospital. He also exhibited an eight months' foetus with general anasarca and curious folds on the skin.

The Section then adjourned.

**SURGICAL SECTION.**

President—**SIR CHARLES A. CAMERON**, President of the Royal College of Surgeons, Ireland.

Sectional Secretary—**WILLIAM STOKES, M.D., F.R.C.S.I.**

*Friday, December 11, 1885.*

The **PRESIDENT** in the Chair.

*The Surgery of the Suakim Expedition.*

**MR. TOBIN**, late of the Army Medical Department, read a paper on the above subject. He explained the nature of the appointment styled "Field Surgeon" (which was that held at Suakim by himself), and pointed out the necessity for its being made a recognised appointment and its duties defined by the regulations. The statistics of the expedition were given, from which it appeared that of 129 wounded 124 recovered, or 97 per cent. Having indicated that to turn out cured in England a patient wounded at Suakim is a highly complex undertaking, requiring much forethought on the part of the administrative authorities and skilful carrying out of their plans by every branch of the Executive, the writer asked the members of the Academy to narrowly criticise the statistics submitted, as he believed such criticisms would show the British taxpayer that he had got value for his money, and encourage him to further outlay on the Army Medical Department and its equipment. Having exhibited a femur fractured by a rifle bullet at a point remote from that struck, the writer next pointed out the great difference noticed during the campaign between wounds from the Martini-Henry and Remington rifles. By referring to experiments of Professor Longmore, he showed that experiments on the dead body afford a reliable means of ascertaining the effects of gunshot wounds on living tissues, and that therefore the extension of such experiments was a desideratum. Cases illustrative of wounds caused by the Martini-Henry and Remington bullets, and of remarkable recoveries from injuries to brain, and a reference to the valuable help given by Professor Ogston, of Aberdeen, concluded the paper.

**MR. WHEELER** said there were two or three points which Mr. Tobin had brought prominently forward. First, there was the advantage of having a surgeon who is in the habit of operating accredited to the army on active service. That was an advantage which went without saying. Billroth was attached to the German army during the Franco-Prussian war. He had the local rank of Surgeon-General, and was probably as well paid as most Germans. But if the British taxpayer desired the advantage of such aid for the British army in the field, he would not only

have to pay largely the civil surgeon attached to the army, but make provision for his family should anything untoward happen him. The statistics of the mortality of the wounded were most creditable—out of 129 wounded there being only five deaths recorded; and from the nature of the wounds of the five fatal cases it would have been almost impossible to save life. This very small mortality was due partly to the excellent care which was always afforded to the British soldier by the Army Medical Department, and partly to the very easy transport, the hospital where the operations took place being only about three miles from the seat of action, where the wounded were conveyed by dhoolies. The climatic influences, of which he had personal knowledge (having served some years ago in the country), were combatted by shipping the patients as fast as possible and getting them away to sea. There was also drainage well established in every case, the necessity for which in wounds and after operations had been first definitely laid down by the late Mr. Callender. Moreover, there was easy transport in the Egyptian campaign, the wounded having been conveyed by water from Kassassin. The specimen of the femur exhibited by Surgeon-Major Tobin was unique. He was not aware of any recorded case of a fracture several inches away from where the ball struck. It was evident the ball had grooved the bone between the trochanters, causing splintering, but a fracture of the bone had taken place several inches below the neck of the bone. Dupuytren had written on the splintering of bone, classifying the splinters, and emphasising the impropriety of removing splinters indiscriminately. Professor Longmore's experiments, which Mr. Tobin had alluded to, were exceedingly valuable. The differences in the wounds produced by the bullets from the Martini-Henry and the Remington rifles would be accounted for by the great difference in the charge of powder.

DR. BENNETT valued the communication as showing that much more conservative surgery might be adopted in the treatment of gunshot wounds than had been hitherto the rule. So, where a rifle bullet passed through the shaft of a bone tolerably near a joint, or even remote from it, they were not to conclude that there would be longitudinal splintering into the joint necessitating the amputation of the limb. The immunity which had been pointed out followed from the success of the antiseptic treatment.

MR. THOMSON concurred with Mr. Wheeler in regarding as important elements of the success attending surgery in connection with campaigns—first, the great care bestowed upon the British soldier; secondly, the admirable transport devised in the Suakim expedition; and thirdly, the facility with which the patients were removed from the climatic influences of the place; but from that point Mr. Wheeler and he parted company. It should be remembered that Mr. Tobin, in his paper, had



laid particular stress upon the importance of antiseptic surgery in his own treatment. Mr. Tobin had carried out with him from Netley large experience of the value of that system of surgery; he practised it in the campaign, and he now related the result of his experience. But Mr. Wheeler had quite ignored what Mr. Tobin regarded as exercising the main influence on his success, and had merely mentioned that drainage was important. All agreed as to the importance of drainage, but certainly not in the statement that it was introduced by Mr. Callender; for long before Callender's time drainage was known and practised. The influence of transport upon the success of operations had not been developed in the late campaign, or in recent campaigns, but had been demonstrated so long ago as the Russo-Turkish war, particularly in Asia Minor and in the Erzeroum district, where the transport was of the worst character. But by the Listerian or antiseptic method of treatment, even under conditions in which the patients were exposed to all kinds of hardship, both of climate and transport, the wounds did well.

MR. WHEELER said he did not intend to convey that Mr. Callender had introduced drainage, but that he was the first who had laid great stress on the necessity of drainage in wounds. He adopted antiseptic surgery, but he deprecated what was termed "Listerism" as a thing of the past.

MR. CROLY said the results were due to antiseptic surgery, and it was as unnecessary to speak of antiseptic dressings as that a deep abscess must be opened to let out the matter. Some surgeons argued that an anterior long flap and a short back flap were more likely to slough than others. In civil practice, where the rectangular flap amputation was adopted, sloughing was of rare occurrence; but in gunshot wounds the circular flap amputations were preferred, in order to leave as little skin as possible. It was therefore interesting to find that Mr. Tobin had adopted the former with good results. Mr. Tobin had by his method transmitted his patient, so that the Listerian treatment would be continued.

MR. BAXTER mentioned that in his experience of the Franco-Prussian war the percentage of recoveries was very good, though not so large as Mr. Tobin had recorded. Antiseptic dressings were never used. Ether was the anæsthetic.

MR. TOBIN replied. In reference to Dr. Bennett's remark on the fissuring of the bones, the experiments of Professor Longmore showed that when bullets were fired into long bones the fissuring was often carried down to the capsule of the joint, the synovial cavity being opened into. By rough handling the fissuring would extend into it, but with care the wound was rendered non-penetrating. As to Listerism, he could not say whether Lister's theory was correct or not; but he had it always

before him, and he thought that cleanliness could not be better arrived at than by carrying out the idea which had got the name of "Listerism." If the idea was known before, Lister, at any rate, deserved the credit of having made it current coin. In his operations, a long anterior flap, if easily cut, was the one he found most suitable, as falling over the wound like the cover of a box, letting it go on healing, whether dressed or not, and therefore obviating the necessity of frequent dressings.

*On the Organisation of the Medical Department in an English Army Corps, with suggestions as to Volunteer aid.*

SURGEON-MAJOR EVATT delivered an address on this subject. He said that civil doctors generally knew nothing of our army system of work, and both sections of the profession suffered in consequence. The army surgeon had developed ambulance aid to a point far beyond average civil arrangements, and the systematic training of the students in this work would be useful to all civil doctors. The medical service of the army had worked out a very logical system of hospital administration, based on the principle that the trained medical officer was the best fitted individual to be governor and director, as well as scientific head. Civil doctors, as a rule, had little power in their hospitals, and the students received no training in administration; yet it was a highly important matter, as hospitals went to grief constantly, not from defective science in surgery, but from bad nursing, bad construction, bad drainage, want of discipline and supervision—yet in none of these heads were the students trained. He then explained the war system of the army corps, showing on a diagram the battalion aid, the bearer companies, the field hospitals, the hospitals on the communications, the base hospitals, and the hospital ships—and traced the course of the wounded soldier from the battle-field back again to Dublin. He then said, that with the new duties undertaken by the medical service of the army no change had been made in the student's training. No training for enforcing discipline had been given to the student, no teaching in ambulance aid, and no teaching in hospital administration had as yet been made part of the student's course. He suggested that the students should, as a matter of routine, be drilled in ambulance drill; 2ndly, that definite teaching in hospital administration should be given them, viz.—in cooking, nursing, *personnel*, construction, ventilation, and drainage; and, 3rdly, that residences for students were highly desirable in connexion with the medical schools, to improve the discipline of the student and his social comfort. If students were so trained in the ideas of the military science in time of war, we could utilise civil doctors as aids, with the certainty that they would be disciplined men. Everything went back to the training and the discipline of the student.

MR. CANTLEY, of Charing-cross Hospital, London, advocated the importance of bringing civil surgeons into "touch" with the Army Medical Department, by the means which Surgeon-Major Evatt had suggested, and stated that he had himself organised a volunteer corps of medical students.

DR. BARTON considered the advantage of the training to medical students would be very great, but, while willing to join in the project, felt unable to decide at once as to its practicability.

MR. DOYLE was in favour of having a man thoroughly trained, especially in hospital administration, if it could be taught, but allowance should be made, or the student's time extended for that purpose.

DR. MYLES said the medical student was the most overworked creature upon earth, and an hour's football would do him more good than ambulance drill. As secretary of the Dublin Hospital Commission, he was enabled to say there was no system of administration existing which could be taught by the medical men

MR. THOMSON said it was impossible that Dublin medical men could be conversant with all the details of hospital administration, as they had practically nothing to say to the management of the institutions with which they were connected, the system being that the management was vested in the hands of lay persons, who knew nothing of it, and took care to keep the doctors, who knew most, out of it. As to Surgeon-Major Evatt's scheme, he did not understand exactly what he proposed—for instance, in what way the medical student who had no intention of entering the Army Medical Service would be useful? There was no volunteer or reserve force in Ireland, and until such a force was established, it was rather a vague benefit to train a number of medical students in ambulance exercise. If the Government wanted men, the Government ought to ask for them.

SURGEON-MAJOR EVATT said the training would be useful to medical men going to the Colonies or volunteering in case of war.

The Section adjourned.

# MEDICAL SECTION.

President—F. R. CRUISE, M.D.; President and Fellow, King and Queen's College of Physicians.

Sectional Secretary—A. N. MONTGOMERY, M.K.Q.C.P.

*Friday, December 18, 1885.*

The PRESIDENT in the Chair.

## *Living Specimens.*

DR. R. A. HAYES exhibited a case of paralysis of the left recurrent nerve.—Dr. C. F. MOORE showed an infant suffering from pemphigus.

## *Specimens by Card.*

DR. J. MAGEE FINNY exhibited an ulcer of the stomach perforating the left ventricle of the heart, and causing death by hæmorrhage.

## *On the Quantitative Determination of Albumen and Urea in Urine.*

DR. CRUISE read a communication on the above subject. The simple clinical methods which he demonstrated were those of Dr. G. Esbach, chief of the chemical laboratory at the Hôpital Necker, Paris. The instruments used were those constructed by Brewer, frères, of Paris. For the quantitative determination of albumen, Dr. Esbach uses a tube, graduated in accordance with the result of experiment. Into this tube a definite quantity of urine is introduced, and then a definite amount of solution of picric and citric acids. After 24 hours' rest the deposit which has fallen is read off by the graduations, which give the amount of albumen in grammes per litre. These manipulations occupy about one minute. Dr. Esbach's method of determining the amount of urea in urine is founded on that of Dr. Edmund W. Davy, of Dublin, and published in 1854. He takes a glass tube, 15 inches long, closed at one end, graduated in cubic centimètres and millimètres. First he introduces and about 8 cubic centimètres of a rather strong solution of hypobromite of soda, adding distilled water until the mixture reaches 140 millimètres. Next he takes one cubic centimètre of the urine, and having discharged it rapidly into the tube, closes the latter promptly with his thumb, and by a few movements of inversion thoroughly mixes the contents. Decomposition sets in at once, and a considerable froth is developed. As soon as this subsides he plunges the end of the tube, closed by the thumb, into a water-bath; then he removes the thumb. The fluid is rapidly lowered, being expelled by the nitrogen evolved. When all is steady he

again closes the tube with his thumb, under water, removes it from the bath, and holding it upright reads off the amount of fluid left. This will be less than the 140 millimètres with which he commenced operations, and the difference between the two amounts gives the volume of nitrogen. The next point is to estimate the amount of urea from the volume of nitrogen, making allowance for the state of gaseous tension at the moment. Dr. Esbach provides us with an easy method of accomplishing this without any calculation, as follows:—First, he provides us with an ingenious instrument which he terms a baroscope, which indicates gaseous tension, according to the existing temperature; and, secondly, he gives us a set of tables constructed on the basis of the two figures we have—namely, the volume of nitrogen, and the number indicated by the baroscope. With the aid of these two items, and the tables, we can read off instantly the quantity of urea per litre. The entire process occupies about five minutes. Dr. Cruise dwelt on the importance of making frequent quantitative analyses of albumen and urea, as well as on the facility with which a fairly accurate result may be obtained by the methods he exhibited.

DR. QUINLAN testified to the difficulty of determining the quantity of albumen and urea in urine.

DR. A. W. FOOT having used Esbach's test for two or three years, found one source of dissatisfaction with it. Esbach's direction was to read the number of grammes per litre on a graduated scale after allowing the urine and reagent to stand for 24 hours. But in 24 hours more a very much less degree would be registered. So far as he had read, Esbach had not noticed the fact that the precipitate condensed day after day into smaller bulk.

DR. TICHBORNE did not think this objection had been proved. The test was an approximation for practical purposes, and Dr. Esbach had graduated his tubes to be read after standing a given time. Of course precipitates standing would gradually get more dense, but the tubes were graduated on the understanding that they would be read after standing 24 hours. When picric acid came out he tried some experiments as a qualitative test against the old nitric acid test, which, when properly applied, was one of the best. When poured down the side of the test-tube, he found the nitric acid test was slightly more sensitive than picric acid. At the same time, picric acid had the great advantage that when visiting it was easily carried in one's pocket. In urine there were occasionally albuminoid bodies that could not be got at by the ordinary methods of coagulation or boiling.

MR. J. A. SCOTT pointed out, as an objection in connection with picric acid, that other things besides albumen came down in the cold solution if added to it—for instance, where the patient was taking quinine. In estimating the quantity of urea there was some trouble in calculating

according to the tables supplied. Other methods enabled an estimate to be made by percentages, thus dispensing with bookwork.

DR. WALTER G. SMITH recognised, as an occasional difficulty in the use of picric acid for quantitative analysis, that it precipitated other substances, notably quinine, resinous acid, peptones. Tests as to its delicacy compared with the older method of estimation of albumen by weighing, which was more accurate, had been made in Christiania, Germany, and England. He had himself, twelve years ago, in the Adelaide Hospital, tried picric acid as a qualitative test, and he saw no reason for preferring it to the better known and more old-fashioned test which had not been yet dispossessed from practice. The experiments in Christiania by Professor Laache showed that the most delicate qualitative test was the old nitric acid test, and accordingly Heller's method of using nitric acid as a test had maintained its pre-eminence for the recognition of small amounts of albumen. He asked how long would the hypobromite solution keep, as sooner or later it would pass into the state of bromide of sodium? The question was how soon or how late? To render the test of value, the solution should be freshly prepared, and not more than a few days old. If the solution had to be made every few days it would take away from the readiness of the test, which was its chief recommendation. The experiments at Christiania showed that it did not give more than an error of about 1 per cent. lower than the results obtained.

DR. FALKNER said the test might be clinically correct as regards the state of albumen in the urine, but was not absolutely correct. The nearest approach to a correct quantitative clinical test was Olliver's method, whose basis was also citric and picric acid, the quantity of albumen being estimated by the disappearing point of a dark line which was placed behind a test-tube of known dimensions. With regard to urea, Professor Emerson Reynolds had some years ago made a complete apparatus for estimating the amount of urea, by measuring the gas, not the quantity of water displaced by the gas evolved, and he gave a table of calculations.

DR. FINNY said he had tried Esbach's method, and not considering it useful even in the way of saving time, he had discontinued it. Indeed it was no safer than the old method of boiling the urine and letting it stand. There was one great objection to its use which he had found in testing urines—namely, that where there was a large amount of albumen the test lost much of its readiness as a clinical method, as it had to be so frequently repeated before the urine could be sufficiently diluted to render it an accurate test for albumen. Having tried both picric and nitric acid, he found that in the majority of cases the nitric acid test was the most accurate and most reliable. Dr. Tichborne had mentioned, no doubt it was easier to carry a solution of picric acid than nitric acid ;

but where accuracy was required about a small quantity of urine, nitric acid was the safest and best thing to use.

The PRESIDENT replied. He, too, like Dr. Foot, had observed the progressive condensation of the coagulum. According to Esbach's notion, the reading should be made after 24 hours. As to Dr. Tichborne's question about the albuminoids, it was true a certain amount of confusion and difficulty would arise, but the method sufficed for general clinical purposes without seeking scientific accuracy. The same difficulty had been raised by Dr. Scott with regard to quinine and peptones, but when it was known the patient had taken quinine allowance could be made. There was no real difficulty in reading from the tables the results in the urea test, as where the two numbers crossed it could be seen at a glance. It was quite true, as Dr. Smith had indicated, that other substances were precipitated by picric acid. Urates were best got rid of by careful filtering. He had been using the hypobromite solution for some months and it had kept. Whenever he had doubt of the solution he added a little more; thereupon, if it effervesced or fresh decomposition set in, it was clear there had not been sufficient; or if the solution lost any of its olive oil colour he added a little bromine. The absolute quantity of the hypobromine solution was unimportant, provided it was rather in excess. He had tried Olliver's test for the quantitative analysis of urine and of sugar, and he found it difficult to determine from the obscurity of the light the exact point. Besides Olliver gave only a percentage from which it was necessary to calculate back, and his test of sugar was still more difficult. As to the principle of Dr. Emerson Reynolds' method being the measurement of the water instead of the gas, that was a mere point of detail. The same principle was adopted by Dr. Squibbs, Parke Davis, Scott, and others. The great advantage Esbach's instrument possessed was that the analysis could be made within five minutes, and probably with as much accuracy as any other. The point raised by Dr. Finny as to the difficulty where the urine was strongly albuminous was met by diluting it to half or quarter strength, and making allowance for the calculation.

*On a Case of Partial Embolism of the Inferior Division of the Central Artery of the Retina associated with repeated attacks of Chorea.*

MR. ARTHUR BENSON read a paper on the above subject. [It will be found at page 117.] The patient, a house painter, aged twenty-one, had had rheumatic fever seven years ago. Three years ago had an attack of left hemichorea. Two years ago another attack of more general chorea, and last year a third attack of chorea.

On October 14th, 1885, he suddenly became blind of the right eye; in a few minutes vision began to clear from below upwards, and in fifteen minutes the lower half had quite cleared to the horizontal line, passing

through the fixation point. There the improvement abruptly ceased, and has not since proceeded. The ophthalmoscope showed œdema of the lower half of the retina, and "a cherry-red spot" at the macula. The vessels (veins and arteries) were all filled with blood, and pulsation could be produced with pressure, showing circulation had been re-established at that time (*i.e.*, fifteen hours after the eye got blind). The vessels of the lower half of the fundus were not quite so plumply filled as those of the upper half. In a few days there were evidences of atrophy of the disc and shrinking of the retinal vessels in its lower half; and as the œdema of the retina diminished, the atrophic changes increased and the vessels shrank markedly. The diagnosis was that an embolus for a time partially blocked the central artery before its bifurcation, and subsequently got shifted into the inferior division and partially blocked it, for the vessels were all carrying blood in the right direction, showing that the obstruction was not complete. He had never had any similar attacks. His heart and kidneys were carefully examined and nothing abnormal discovered. He was in other respects healthy, and there was no evidence to show where the embolus came from. The case was of interest as bearing upon the embolic theory of the pathology of chorea. Drawings of the ophthalmoscopic appearances and charts of the field of vision were exhibited.

DR. NIXON said there had been no suggestion where the embolism came from. The patient was stated to have had a history of an attack of rheumatic fever, yet the heart was perfectly sound. However, the fact that the patient had had an attack of hemichorea in the left side, and that he got an affection of the right retina, clearly showed that the disease was embolic, and it was difficult to say where it came from except from the heart. Where there was no peripheral disease with which to connect the embolism, it must be assumed that it came from the heart.

DR. PURSER observed, as regards Dr. Nixon's assumption that the existence of embolism necessarily proved disease of the heart, that not one out of fifty cases came from the valves of the heart. In a great many cases the deposits on the valves of the heart were too firm to be washed off. It was much more likely to have resulted from thrombi which became broken up and were carried away in the current of the circulation. The examination of the emboli showed that they do not come from the valves of the heart, but from coagula in the blood-vessels.

DR. FOOT said the patient had been under his care affected with hemichorea on the left side, in his arm, then the leg, and lastly the face. At that time he had no cardiac murmur, but he gave a history of rheumatism, and that he had had a rheumatic ailment about nine months before the attack of hemichorea. It was then the fashion to treat chorea hypodermically with arsenic, and the patient was injected with arsenic to the



masthead twenty-six times, beginning with five and going up to thirty minims. At fifteen the strength was doubled to minimise the bulk, nor did he stop until he gave  $\frac{1}{2}$  gr. of arsenic under the skin. He believed that, that excellent medicine, camphorated water, would have done as well.

DR. BENSON, in reply, said he had seen but a very small percentage where a distinct heart lesion was discoverable.

The Section adjourned.

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## PATHOLOGICAL SECTION.

President—T. EVELYN LITTLE, M.D.

Sectional Secretary—J. B. STORY, F.R.C.S.I.

*Friday, December 4th, 1885.*

The PRESIDENT in the Chair.

### *Madura Foot.*

DR. E. H. BENNETT exhibited a specimen of Madura foot which had been presented to the museum of the University of Dublin by Surgeon Burke, of the Indian Medical Service, a former pupil of the School of Physic, and a graduate of the University. The specimen was obtained by amputation for the relief of the disease, which occurred in a coloured woman, an inhabitant of Central India.

Dr. Bennett said that he could add nothing to the description of the disease which had been so fully given by Carter in his splendid monograph. He would simply point out the close likeness of this specimen to that represented in the plate in Carter's "Mycetoma." Although the general features of the specimen agreed so closely with those of the plate referred to, there existed some difficulty in deciding to which of the varieties of the disease—the "melanic" or the "ochreous"—it should be referred, for the action of alcohol in preserving the specimen had blanched the fungus particles, so that their colour had been lost. This change was increased by the defective preserving power of the native Indian spirit, which had imperfectly penetrated the specimen, and, perhaps, further by the change to strong spirit here. On first examination of the fistulous openings which abounded all over the misshapen member, some few dark grains of fungus growth could be discerned, which led him to refer the specimen to the melanic variety.

DR. HENRY KENNEDY asked was the disease chronic and did the patient suffer much.

PROFESSOR BENNETT said the disease was extremely painful—at all events in the present case it was sufficiently so as to compel the woman to allow her foot to be amputated by a military surgeon.

*Milky Fluid from a Case of Ascites.*

DR. DUFFEY exhibited a specimen of a milky fluid removed by paracentesis during life from the peritoneal cavity of a single woman, aged fifty-two; and also portions of the viscera from her body. He briefly referred to the different forms in which a fluid resembling milk is met with pathologically, and said that he believed many of the so-called cases of "chylous" ascites were so only in name. In Dr. Duffey's patient the fluid was certainly not chylous. The case was one of chronic diarrhoea of five months' duration during the last six weeks of which there was, in addition, ascites. The abdomen was tapped three times. On each occasion a milky fluid was withdrawn. Erysipelas supervened after the last tapping, and the woman, who had become very weak and emaciated, died. On *post mortem* examination the parietal peritoneum was found to be thickened and opaque, and, as well as the costal pleura, sprinkled over with small, white, fibrous nodules, which were likewise plentifully bestrewed over the greatly thickened omentum, and the serous envelope of the intestine. In Douglas's pouch there was a considerable deposit of nodules, which, in some places, had coalesced and looked like caseous masses commencing to soften. There were enlarged glands in the lesser omentum and in the vicinity of the cæcum. Numerous small ulcers were found in the cæcum itself and adjoining portion of the large intestine; and both segments of the valve were much thickened and ulcerated. The thoracic duct and its branches appeared quite normal. Dr. Duffey believed the case to be one of tuberculosis, but he could not say what was the cause of the colour of fluid, or what connection, if any, there was between it and the chronic peritonitis. So far as these two circumstances went, the case seemed to support the conclusion recently advanced by M. Letulle (*Rev. de Méd.*, Sept., 1884), to the effect that a chronic inflammatory element is the most constant character in the genesis of such chyloform effusions.

After some observations from DR. FALKNER—

DR. FOOT said he had a case of this kind in which he tapped the patient four times for effusion into the peritoneal cavity. Mr. Scott analysed the fluid and found in it 25 per cent. of sugar, and a notable quantity of urea, which he expected, as the man was suffering from parenchymatous nephritis and general dropsy. But his limbs and scrotum had to be punctured—which was done without any bad results—and the fluid which issued from these punctures, of which he made about 150, was as limpid as dew. On the other hand the fluid that came from the peritoneal cavity was like milk and water; and similar fluid came up from the man's stomach. By the four tappings twenty-four quarts of this milky fluid were taken from him. The specific gravity of it was from 1·004 to 1·006. He determined the white colour to be due to white blood-cells, granules, and corpuscles which had been originated by the chronic inflammation of the

peritoneal cavity. The man had had hepatitis, having been addicted to drink, and also got disease of the kidneys.

DR. HENRY KENNEDY said he believed the disease in the present case partook of the nature of a disease described many years since by Barron. He had seen other cases like it, and in one of them the peritoneum was still further advanced in disease. Barron gave an admirable account of the disease, which he said was neither tubercle nor carcinoma, and described it as tubercular accretions.

PROFESSOR BENNETT remarked that if the milky colour was due to white blood corpuscles the fluid would not remain so thoroughly mixed as it was in the bottle, nor would it have stood the test of filtration.

MR. STORY asked did Dr. Duffey find any microscopic evidence of the presence of bacilli.

DR. FOOT said that in the case which he himself had mentioned, an examination was made of the man's eyes, but nothing of diagnostic value was found.

THE PRESIDENT remarked that after two or three tapplings the man, whose case was the subject of Dr. Duffey's communication, got an attack of erysipelas which assumed a gangrenous character, and perhaps was the cause of his death. The supervention of erysipelas was very rare after tapping for ascites. Where did it start from in the case in question?

DR. DUFFEY, in reply, said he believed most analyses that had been made of the kind of milky fluid in question had shown the presence of sugar. In his case the fluid had not been examined for sugar. Dr. Foot's case bore out the view that a great many cases of the kind were due to chronic peritonitis. A microscopic examination of a portion of the fluid drawn off at the second tapping showed some endothelial cells and exudation corpuscles. There was no microscopic evidence of bacilli—Dr. Purser and other gentlemen had detected none. But there was a case of convalescent erysipelas in the ward, and in consequence every precaution as to cleanliness of instruments and disinfecting was taken. The erysipelas started from the site of the third puncture made in the patient, gradually extended, and in a short time assumed a bluish appearance. There was no vesication, and the erysipelas did not extend into the peritoneal cavity.

#### *Tumour of the Internal Saphena Vein.*

DR. J. K. BARTON made a communication on a tumour of the internal saphena vein. It was about the size of a pullet's egg, and was evidently formed by an expansion of the coats of the vessel. It was filled with a clot so firm and adherent to the lining membrane that it could with difficulty be separated.

DR. HENRY KENNEDY remarked that several years ago he saw some remarkable tumours in the insides of the veins of cattle which had been

attacked with pleuro-pneumonia. Where the animal lived for six or eight weeks the disease ran into phthisis, with an immense deposit of tubercle, and the great veins leading to the heart were obstructed with distinct tumours adherent to the coats of the veins and as large as marbles. At the time he presented to the Museum of the College an extraordinary specimen, in which extensive disease occurred in the veins of both groins, and all the deep veins, including the femoral and its branches, were turned into bone. Both legs of the individual were affected with a horny disease.

DR. MACSWINEY regarded the case submitted to the section as one of a venous thrombus occurring in a man of feeble circulation and sedentary habits, which, instead of breaking up and being carried through the circulation and creating embolus, hardened and coagulated.

DR. BARTON, in reply, said the specimen exhibited a varicose condition of the vein of more than ordinary interest from the exceedingly deformed character and aneurysmal appearance.

#### *Case of Bright's Disease.*

DR. FOOT exhibited the heart and kidney from a case of Bright's disease. The kidneys were typical examples of the contracted granular kidney, as described by Richard Bright. The heart was hypertrophied, weighing 32 ozs., the hypertrophy chiefly affecting the left ventricle. Neither valvular disease nor atheroma of the aorta was present, but the surface of the heart was covered with a regular sheet of pericardial exudation. The chief clinical feature in the case was the presence of persistent hæmatemesis.

#### *Rupture of an Intercranial Vessel.*

DR. MACSWINEY exhibited the brain of a man who had died from rupture of an intercranial vessel. The man, aged 60, was brought to the hospital that day week in an insensible condition. He was absolutely unconscious, immovable, and without sign of sensibility. He was a labourer and was actually at work that day. A few hours before the fatal attack he complained of pain in his head. That did not deter him from continuing his work, and he was in the act of levelling a wall with a crowbar when he fell. When persons came to his assistance he was conscious and could speak, but was unable to walk; but consciousness and power of speech rapidly left him, and in two hours after the attack, when he was brought to the hospital, he was in the state which he (Dr. MacSwiney) had described. His face was pale and his eyes contracted and immovable, but he had not stertorous breathing. His pulse was feeble and rapid. The sphincter of his bladder was paralysed, and he passed a quantity of urine which trickled to the floor. His left upper and lower extremities were extremely rigid and almost tetanically con-

tracted. He lived about fourteen hours after his first seizure. After death the brain was removed. The convolutions on the left side were very much flattened, and there was considerable subarachnoid effusion of blood on the surface of the convolutions. The base of the brain was occupied by a very large effusion of blood. It appeared that the blood burst through the arachnoid, and was effused into the arachnoid space. When the loose blood had been removed it was found that on the left side there was an aneurysmal dilatation of one of the branches of the internal cerebral artery which had given way. There was a very large hæmorrhagic focus, which had torn through the white substance of the frontal convolutions and excavated a large cavity which was filled with clotted blood, and the sides of which were formed of projections of lacerated brain substance. The ventricles had not been opened, but he had no doubt that there was blood in them. Arterial hæmorrhage in the brain generally resulted from the rupture of a diseased vessel; and, in the present instance, the diseased vessel showed the form of degeneration known as atheroma, which was not the most usual form that existed in the arterial dilatations described by Charcot. He had no doubt that the efforts the man was making were the cause of the rupture.

#### *Osteomalacia.*

DR. E. H. BENNETT exhibited specimens taken from a patient who had been under his observation for several years before her death, and during the entire period of her disease. The case was exceptional in this, that while the woman was comparatively young, being at her death about thirty-nine years of age, pregnancy had not occurred as the starting point of the disease—nor were the conditions of life such as could be assigned as the cause. She had lived in the capacity of upper nurse in most comfortable situations and in various climates, in England, Ireland, and in France. The chain of events in the progress of the disease was—spontaneous fracture of the left clavicle, after union of this its refracture, a fracture of the shaft of the humerus at its upper extremity, followed at intervals of several months by successive fractures of the same bone in its upper half, each fracture uniting well and in the usual time for healthy bones. During the treatment of the last of these accidents, just as the repair was completed, the shaft of the femur on the same side broke in its upper half as the patient turned in bed. At this time she suffered severe pains in the lower limbs and back, and particularly in the right thigh—such that one began to expect the occurrence of fracture of that bone also—she could not bear the restraint of a Liston splint and milder means were adopted, but the patient suddenly died from failure of the heart. Her fingers had become remarkably clubbed at the ends, while the nails were corrugated, points which raised a suspicion of pulmonary phthisis, but of this there were no signs during life and no evidence *post mortem*. The

bones which had been the seat of pain as well as those that were broken, presented a red mottled appearance, and were so softened that a scalpel could easily be thrust through their tissue, even through the shaft of the unbroken femur. The medulla was, however, fatty and free from red colorisation. The kidneys were studded everywhere with fine grains of dull white particles of gritty sand, against which the edge of the knife grated in cutting the cortical and medullary tissues, but there was no free-sand or calculus in the hilum, ureters, or bladder. Chemically examined these grains were found to be composed of mixed phosphate and carbonate of lime—to be, in fact, bone-earth. Structurally they were amorphous. The liver and spleen were free of disease. The heart was very small, thin, and soft, and evidently fatty. Dr. Bennett discussed the relation of this affection to rickets, and to senile osteoporosis, pointing out the characters which appeared to him to distinguish them.

DR. BYRNE remarked that their museum was so deficient in specimens of osseous disease that it was well worth while to visit Dr. Bennett's museum. At first he (Dr. Byrne) thought the present case one of fragility of bones, depending on fatty degeneration, such as was described by Paget. Generally, one of the remarkable features of the disease was the complication of the pelvis. Dr. Woods had pointed out that the pelvis and the pubic bones were the first to show any appearance of the disease. In the case of Madame Supio the pelvis was twisted up and reduced to a deformed mass; but it was found, after her death, that the osseous system had, to a considerable extent, recovered its natural condition. In the present case the woman appeared to have died without any attempt at recuperation.

PROFESSOR BENNETT, in reply, said, in the present case a tendency to repair had shown itself at the seat of the fracture, while the disease progressed elsewhere; whereas it was one of the characteristics usually met with in osteomalacia that the fractures did not unite. Here a repair took place, and yet the repairing material subsequently underwent degeneration.

The Section then adjourned.

# SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P., F. R. Met. Soc.

## VITAL STATISTICS

*Of the Eight Largest Towns in Ireland, for four Weeks ending Saturday,  
January 2, 1886.*

Towns	Population in 1885	Births Registered	DEATHS REGISTERED			DEATHS FROM SEVEN ZYMOTIC DISEASES							Deaths from Phthisis	DEATH-RATE per 1,000	
			Total Number	Under 1 year	At 60 years and upwards	Smallpox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhoea		From all causes	From seven Zymotics
Dublin, -	353,082	792	848	176	204	-	2	10	1	50	24	12	83	31·3	3·7
Belfast, -	219,222	544	394	76	63	-	6	9	2	5	12	10	57	23·4	2·6
Cork, -	80,124	141	175	18	53	-	2	-	-	-	3	2	26	28·4	2·3
Limerick, -	38,562	74	76	10	23	-	-	-	-	1	1	-	6	25·6	0·7
Derry, -	29,162	70	68	11	14	-	-	-	-	3	3	-	3	30·3	2·7
Waterford, -	22,457	35	34	9	12	-	-	-	-	-	-	2	5	19·7	1·2
Galway, -	15,471	30	37	4	15	-	-	-	-	1	1	-	4	31·1	1·7
Newry, -	14,808	32	18	4	5	-	-	-	-	-	-	-	3	15·8	—

### Remarks.

In the eight selected towns included in the foregoing Table the highest death-rates are 31·3 per 1,000 of the population annually in Dublin, 31·1 in Galway, 30·3 in Londonderry, and 28·4 in Cork; the lowest rates are 15·8 in Newry, 19·7 in Waterford, 23·4 in Belfast, and 25·6 in Limerick. The rate of mortality from seven chief zymotics ranged from 3·7 per 1,000 per annum in Dublin, 2·7 in Derry, 2·6 in Belfast, 2·3 in Cork, 1·7 in Galway, 1·2 in Waterford, and 0·7 in Limerick to *nil* in Newry.

The recorded deaths represent a rate per 1,000 of the population annually of 22·0 in twenty-eight large English towns (including London, in which the rate was 21·3), 26·6 in the sixteen chief towns of Ireland, 25·7 in Glasgow, and 21·2 in Edinburgh. With the advancing season, there is once more a marked increase (from 20·2 to 22·0) in the mortality in the English towns generally; in London it has risen rather less decidedly, namely, from 20·0 to 21·3 per 1,000 per annum. The mortality in the metropolis was each week 18·8, 22·0, 18·0, and 26·5 per

1,000 per annum respectively. The death-rate fell slightly in Glasgow (from 26·0 to 25·7). In Edinburgh, however, it rose slightly (from 20·6 to 21·2). In the Irish towns the rate of mortality has again increased decidedly, from 23·7 to 26·6. If the deaths (numbering 25) of persons admitted into public institutions from localities outside the Dublin Registration District are deducted, the death-rate of that district becomes 30·4, while that of the portion of the district included within the municipal boundary is 33·5 per 1,000 per annum. In London the mortality from smallpox is nearly at a standstill—the deaths were 10 compared with 8, 14, 29, 29, 63, 105, 193, 239, 179, 109, 197, and 229 in the twelve preceding periods of four weeks each. The weekly number of deaths from this disease since the beginning of the year 1885 have been 56, 42, 70, 58, 59, 60, 56, 44, 37, 27, 35, 23, 24, 39, 40, 49, 51, 48, 66, 75, 50, 55, 62, 39, 37, 28, 39, 13, 15, 18, 16, 14, 15, 5, 9, 6, 9, 8, 11, 7, 3, 0, 0, 9, 5, 3, 2, 2, 1, 3, 0, 1, and 6 respectively. The deaths from diarrhoeal diseases in the same city, which numbered 54, 51, 52, 78, 415, 1,163, 429, 166, 73, and 51, in the ten preceding periods of four weeks each, were 52.

Acute febrile zymotics were returned as the cause of death in 114 instances in the Dublin district, compared with a ten-years' average of 115·9 in the corresponding period and 86 in the previous four weeks. The 114 deaths included 2 from measles, 10 from scarlet fever, 24 from "fever," 50 from whooping-cough, 12 from diarrhoeal diseases, and 1 from diphtheria. There was no death from smallpox. The fatality of scarlet fever shows a satisfactory decrease, the deaths being 4 fewer than in the previous four weeks. Of the 24 deaths referred to "fever," 3 only were ascribed to typhus, while 18 were attributed to enteric fever, and in 3 instances the exact nature of the fever either was not specified or was ill defined. The deaths from fever were 6 more than those registered (18) in the previous four weeks. Six children aged between one and five years succumbed to scarlet fever, and only one victim to this disease was under one year old. The deaths from whooping-cough rose from 27 in the previous four weeks to 50. Of the 50 victims of this disease, 32 were between one and five years of age, and 15 were infants of less than twelve months old. One of the 2 victims of measles was a child aged between 1 and 12 months. Two deaths from cerebro-spinal fever were registered in the South City Districts.

Measles caused 6 deaths in Belfast, compared with 4, 6, 6, 7, 38, 88, and 162 in the seven preceding periods. Scarlet fever was fatal in 9 instances in Belfast, and in 7 cases in Cork.

Diarrhoeal diseases were credited with 26 deaths in the eight towns, compared with 22, 28, 56, 96, 68, 30, 32, 27, and 18 in the nine previous periods of four weeks. In London the weekly registered deaths from diarrhoeal diseases were 16, 6, 13, and 17 respectively.



In the Dublin Registration District 792 births and 848 deaths were registered, compared with 749 births and 743 deaths in the previous four weeks. The births were those of 413 boys and 379 girls. The deaths of infants under one year were 176 against 128 in the previous four weeks; those of persons aged 60 years and upwards were 204, compared with 192 in the previous period.

The deaths referred to pulmonary consumption in the eight towns were 187, compared with 176, 146, 156, 197, 209, 213, 243, 277, 252, 270, 244, and 239 in the twelve preceding periods of four weeks each. In Dublin diseases of the respiratory organs are stated to have caused 190 deaths, against an average of 234.5 in the corresponding four weeks of the previous ten years, and compared with 155, 142, 96, 98, 65, 117, 120, 177, 207, 246, 225, and 213 in the twelve preceding periods of four weeks each. The 190 deaths included 121 from bronchitis (average = 172.2) and 34 from pneumonia (average = 30.3). Of the 121 persons who succumbed to bronchitis, 31 were infants under twelve months, whereas no fewer than 40 had passed their sixtieth year.

On Saturday, January 2, 1886, there were under treatment in the principal Dublin hospitals no cases of smallpox or of measles, 22 cases of scarlet fever, 16 of typhus, 29 of enteric fever, and 10 of pneumonia.

The mean temperature of the four weeks was 42.1° in Dublin, 41.1° in Belfast, 43.9° at Roche's Point, Co. Cork, 39.1° at Edinburgh, 39.5° at Glasgow, and 38.7° at Greenwich. The minimal readings of the thermometer in the screen were 24.3° in Dublin, 27° at Belfast, 30° at Cork, 20.0° at Edinburgh, 15.0° at Glasgow, and 23.3° at Greenwich. The maximal temperatures were 57.7° in Dublin, 56° at Belfast, 55° at Cork, 55.0° at Edinburgh, 57.0° at Glasgow, and 51.5° at Greenwich.

The mean temperature was considerably below the average at all stations during the first week of the period, particularly at the Scottish stations (M.T. = 29.0° at Glasgow and 30.0° at Edinburgh); but in the other weeks the weather was mild, particularly in the second week, when the mean temperature in Dublin was 48.0°, at Cork 48.9°, and even at Edinburgh 45.7°.

#### METEOROLOGY.

*Abstract of Observations made in the City of Dublin, Lat. 53° 20' N., Long. 6° 15' W., for the Month of December, 1885.*

Mean Height of Barometer,	-	-	30.184 inches.
Maximal Height of Barometer (on 23rd, at 9 a.m.) -			30.657 „
Minimal Height of Barometer (on 3rd, at 11 p.m.),			29.170 „
Mean Dry-bulb Temperature,	-	-	41.9°.
Mean Wet-bulb Temperature,	-	-	39.9°.
Mean Dew-point Temperature,	-	-	37.3°.

Mean Elastic Force (Tension) of Aqueous Vapour, -	·229 inch.
Mean Humidity, - - - - -	84·0 per cent.
Highest Temperature in Shade (on 16th), - - -	57·7°.
Lowest Temperature in Shade (on 11th), - - -	24·3°.
Lowest Temperature on Grass (Radiation) (on 11th),	20·1°.
Mean Amount of Cloud, - - - - -	66·3 per cent.
Rainfall (on 10 days), - - - - -	·742 inch.
Greatest Daily Rainfall (on 5th), - - - - -	·161 inch.
General Directions of Wind, - - - - -	W., W.S.W., W.N.W.

*Remarks.*

Very changeable as regards temperature, but generally a fine although cloudy month. Both the rainfall and the rainy days were much below the average, while atmospherical pressure was considerably, and temperature was perceptibly, above it. A sharp frost prevailed between the 7th and the 12th, and another brief cold spell occurred after the 27th.

The mean height of the barometer was 30·184 inches, or 0·302 inch above the average value for December—namely, 29·882 inches. The mercury rose to 30·657 inches at 9 a.m. of the 23rd, and sank to 29·170 inches at 11 p.m. of the 3rd. The observed range of atmospherical pressure was, therefore, 1·487 inches—slightly less than an inch and a half. The mean temperature deduced from daily readings of the dry bulb thermometer at 9 a.m. and 9 p.m. was 41·9°, or 3·7° below the value for November, 1885; that calculated by Kaemtz's formula—viz.,  $\text{min.} + (\text{max.} - \text{min.} \times \cdot 41) = \text{Mean Temp.}$ —from the means of the daily maxima and minima was 41·2°, or 0·7° above the average mean temperature for December, calculated in the same way, in the twenty years, 1865–84, inclusive (40·5°). The arithmetical mean of the maximal and minimal readings was 42·0°. On the 16th the thermometer in the screen rose to 57·7°—wind S.W.; on the 11th the temperature fell to 24·3°—wind calm. The minimum on the grass was 20·1° on the same date. The past month was chiefly mild, but there was a spell of frost from the 7th to the 12th, and shorter periods of cold weather occurred towards the end of the month also. The rainfall was only ·742 inch (less than three quarters of an inch), distributed over 10 days. The average rainfall for December in the twenty years, 1865–84, inclusive, was 2·516 inches, and the average number of rainy days was 17·0. The rainfall, therefore, and the rainy days were both very remarkably below the average.

There was hail on the 7th and 28th, and snow or sleet fell on the 10th, 28th, 29th, and 30th. A lunar corona and halo were visible on the 23rd. The atmosphere was foggy on the 5th, 11th, 15th, 18th, 20th, 23rd, and 24th. The barometer was persistently high over the southern portion of the British Islands, the Bay of Biscay, and France, while very exten-

sive and deep areas of low pressure passed eastwards or north-eastwards across Scandinavia.

On Friday, the 4th, the barometer ranged from 28·55 inches, at Nairn, in Scotland, to 30·83 inches at Toulon. Violent storms or gales from S.W. and W. raged over the United Kingdom, and in Dublin temperature rose first to 54·8°, but fell a few hours later to 36·8°.

The week ending Saturday, the 12th, was very cold—severe frost prevailing on several days in nearly all parts of the British Islands. The distribution of atmospherical pressure was mixed—cyclonic and anti-cyclonic. Bright aurora was seen on the night of the 6th in Scotland, where the frost became very intense. Next day the relative humidity was only 63 per cent. at 9 p.m. in Dublin—such dryness of the air being very unusual at midwinter. On the 11th a westerly wind spread over the country, bringing clouds and an increasing temperature.

Singularly mild, open weather held throughout the following week (ending on Saturday, the 19th). An anticyclone lay over France and the South of England, extending at times over Ireland also, while conditions were rather cyclonic to the N. and N.W. of the British Islands. The 13th and 14th were in Dublin mild, fine days. At 4 p.m. of Wednesday, the 16th, there was a beautiful “afterglow,” temperature having previously risen to 57·7°. A succession of dull, damp days followed, so that the ground became quite wet owing to the condensation upon its chilly surface of the vapour carried in the prevailing warm air-current.

During the week ending Saturday, the 26th, quiet, generally open and dry weather was prevalent in most parts of our islands. A sharp frost occurred in Dublin on the night of the 22nd, and next evening a lunar halo and afterwards a lunar corona were seen in connection with a deep depression which passed across the north of Scandinavia on Christmas Eve.

On the 27th, a piercing westerly wind sprang up between an area of high pressure in the south and a remarkably deep depression which rapidly approached the west of Norway from the Norwegian Sea. Near the centre of this disturbance the barometer fell to about 28·2 inches on the evening of the 28th. In the wake of the depression bitter cold set in, and a violent snowstorm occurred in the north of Scotland as well as in parts of Ulster, while showers of hail, sleet and snow were reported from most stations on the 28th and 29th. As this system travelled away, temperature rose quickly again, and the month closed with dull, mild weather nearly everywhere.

RAINFALL IN 1885,  
At 40 Fitzwilliam-square, West, Dublin

Month	Total Depth	Greatest Fall in 24 Hours			Number of Days on which .01 or more fell
	Inches	Depth	Date		
January, - - -	1·617	·321	7th		23
February, - - -	2·812	·920	16th		19
March, - - -	1·530	·305	3rd		13
April, - - -	2·911	·620	21st		16
May, - - -	2·532	·489	19th		23
June, - - -	1·506	·501	7th		8
July, - - -	1·154	·542	19th		10
August, - - -	3·050	1·719	4th		14
September, - - -	2·862	·364	2nd		23
October, - - -	3·500	·994	6th		22
November, - - -	2·398	·876	26th		17
December, - - -	·742	·161	5th		10
Total, - - -	26·614	—	—		198

Although the rainfall was considerably in excess of the measurement for the previous year, 1884, it was decidedly below the average of the 20 years, 1865–84, inclusive—viz., 28·015 inches. It will be remembered that the rainfall in 1884 was very exceptionally small—only 20·467 inches, the only approach to this measurement in Dublin being in 1870, when only 20·859 inches fell. In six of the 20 years in question the rainfall was less than 26 inches, and therefore less than the precipitation in 1885.

In 1885 there were 198 rainy days, or days upon which not less than .01 inch of rain (one-hundredth of an inch) was measured. This was slightly in excess of the average number of rainy days, which was 194·6 in the twenty years, 1865–84, inclusive. Included in the rainy days are 16 on which snow or sleet fell and 37 on which there was hail. In May hail was observed on 11 days and in October on 7 days. Thunder and lightning occurred on only four occasions during the year.

The rainfall was more evenly distributed than in 1884. Thus, 5·959 inches fell on 55 days in the first quarter, 6·949 inches on 47 days in the second, 7·066 inches on 47 days in the third, and 6·640 inches on 49 days in the fourth and last quarter. The preponderance of the fall in the third quarter is more than accounted for by a tropical downpour, amounting to 1·719 inches, which occurred in Dublin and its immediate vicinity on August 4. On November 26 more rain fell (·876 inch) than during the whole of December, when only .742 inch was registered.

J. W. MOORE, M.D. Univ. Dubl., F.R.Met.Soc.

## PERISCOPE.

### "OPUSCULA PRACTICA."

THE "Opuscula Practica" in Part VI. of *The Asclepiad* are very interesting. In the first place, Dr. Richardson refers to a remedial agent which he believes will play as useful a part in surgery as the ethylates and "colloids" originally introduced by him. This is oxalic ether ( $C_2H_2O_4$ ), a colourless liquid, sp. gr. 1.090, boiling at  $183^\circ C.$ , having a pleasant odour, but a decidedly biting taste. It dissolves readily in alcohol and in ethylic ether; it is also soluble in water to the extent of 4 per cent., but after a time water decomposes it, alcohol and oxalic acid being formed. Potash and soda solutions convert it into alcohol and oxalates of the bases, whilst ammonia solution converts it into alcohol and oxamide. When administered hypodermically oxalic ether is decomposed at the point where it is introduced and acts there perhaps exclusively, the action being to coagulate the albuminoid structures, and to produce almost painlessly a free and dry eschar, without marked constitutional disturbance, unless used in excess. Dr. Richardson looks upon oxalic ether as a destroyer of tissue rather than a caustic, and believes it will prove of service in application either by the brush or by needle injection for the removal of morbid vascular growths. It is prepared by the action of oxalic acid upon absolute alcohol. In a previous part of *The Asclepiad* (V., p. 95), Dr. Richardson had pointed out that amyl hydride, known also as "rhigolene," has no local anæsthetic properties except by the cold it produces upon evaporation. In the present part (VI., p. 168) he repeats the substance of a portion of a recent paper in the *Lancet* (January 17) in respect to some compounds with rhigolene which have been found useful. Camphor and spermaceti dissolve in it freely, and a solution made by dissolving one drachm of each of those substances in two fluid ounces of rhigolene may be applied on cotton-wool to burns. The evaporation of the liquid is said to induce instant relief from pain, and there is left a thin film of camphor and spermaceti covering the injured surface. A solution of five grains of iodine in one fluid ounce of rhigolene affords a means of depositing iodine in a minute stage of division on the surface of a fœtid wound. Further, an ammoniated solution, which acts as an antiseptic for inhalation, is prepared by shaking rhigolene with strong solution of ammonia, and after allowing separation to take place decanting off the water. Natural history specimens and similar objects may be preserved by steeping them in such a solution charged with camphor, the camphor being left in the tissues

upon the evaporation of the more volatile constituent. Dr. Richardson also calls attention to the much overlooked value of sponge as a poultice carrier, especially for mustard. After the mustard paste has been made of a smooth and even consistency it should be taken up on a clean sponge, the sponge laid in the centre of a soft white cloth, the corners of which are tied, and the smooth convex side of the sponge is then applied to the surface of the skin. The mustard sponge, warmed again by the fire and slightly moistened, can be applied two or three times, and remains useful for several hours. The sponge can afterwards be easily cleaned in warm water.—*Pharm. Journ.*, May 2, 1885.

#### CEREBRAL TUMOUR REMOVED BY OPERATION.

In the 68th Vol. of the "Medico-Chirurgical Transactions," Dr. A. Hughes Bennett records the particulars of his celebrated case of cerebral tumour. The disease occurred in an otherwise healthy young man, and followed a blow on the left side of the head. The symptoms consisted in convulsive movements on the left side, at first confined to the muscles of the angle of the mouth and to the tongue, subsequently extending to those of the upper and lower limbs. The attacks were sometimes unattended by loss of consciousness, but often culminated in a fully characterised epileptic fit. There were attacks of very severe headache, situated chiefly at the vertex; there were also vomiting and double optic neuritis. The parts which were at first convulsed subsequently became partially paralysed, the loss of motion being most marked in the upper limb. After these symptoms had gradually developed for three years the patient came under Dr. Bennett's care. Medical treatment failed to effect any improvement in the symptoms, and a diagnosis having been made of a tumour in the middle part of the right ascending parietal convolution, the skull was trephined, and the brain at this part exposed. The surface of the hemisphere appeared normal, but immediately below the surface a gliomatous tumour was found, which was removed without any serious difficulty. With the exception of an increase in the paralysis of the right arm no unfavourable symptoms followed the operation. On the contrary, the headache, convulsions, and vomiting all ceased. This favourable condition was maintained for four days, when the discharge from the wound became fœtid. A hernia cerebri began to form, paralysis extended to the left leg, and there was some anæsthesia of the left side. In all other respects the patient was well, and even the optic neuritis had diminished. On the twenty-first day the patient had a rigor, followed by fever due to meningitis, of which he died a week later. It would be impossible for us to reproduce all the details of this truly classical case, or to follow the author in his discussion of the numerous points of interest which it presents. The case would, however, seem fully to justify the following conclusion, with

which the paper ends :—" In conclusion, we would observe that, although unfortunately in this instance life was not permanently preserved, the experience we have gained by this case leads us to the belief that there is an encouraging prospect for the future of cerebral medicine and surgery, and that, as a tumour of the brain can be diagnosed with precision and successfully removed without immediate danger to life, we confidently anticipate that under more favourable circumstances the operation will be performed with lasting benefit to the patient."

J. M. P.

A CASE OF LOCOMOTOR ATAXY WITHOUT DISEASE OF THE POSTERIOR  
COLUMNS OF THE SPINAL CORD.

DR. A. HUGHES BENNETT records in the Clinical Society's "Transactions," Vol. XVIII., the case of a gamekeeper, aged forty-eight, who suffered from most of the symptoms of locomotor ataxy, and was supposed by all the physicians who saw him to be affected with that disease—" There was inco-ordination in the movements of the limbs and chiefly in the lower extremities, with a typical ataxic gait, increased in the dark. There was at the same time no appreciable failure in the gross power of the muscles, nor were these atrophied. The Romberg symptom was present, the patient swaying and losing his balance when his eyes were closed. There were paroxysms of characteristic lancinating pains, and deficient and retarded sensibility in the skin of the lower limbs. The plantar reflexes were diminished, and the knee-jerk phenomena totally abolished." "There was no affection of vision, or trace of optic atrophy. The pupils, although unequal in size, were not paralysed to the stimulus of light or accommodation. There was no abnormality of the vesical or sexual organs, and the disease had only been in existence for nine months." After death the brain and spinal cord were found much congested. Extensive softening existed in the frontal lobes of the cerebrum on both sides, in the cerebellum and in its peduncles. In the medulla oblongata there was a round-celled growth which seemed to infiltrate the tissues without destroying them. To this the absence of bulbar symptoms during life is attributed. There was general thickening of the pia mater of the cord, most marked in the lumbar and dorsal regions, where, to the naked eye, it presented the appearance of numerous small tumours. The posterior roots and, to a less extent, the anterior roots passed through these thickenings, and must have been pressed on, although they did not seem to be structurally altered. The cord itself was unaffected except at one point in the lumbar region, where an extension of the growth in the pia mater along the anterior nerve roots and blood-vessels reached the anterior cornua. The nature of the growth, causing the change in the pia mater and in the medulla oblongata, is very imperfectly described. It is said to have been composed of round cells, and to have been sarcomatous in

character. In his commentary on this case, the author compares it with those described by Dejerine, in which the symptoms of locomotor ataxy were due to lesions of the peripheral nerve-endings in the skin and muscles. He thinks that it shows "that the point at which, in the causation of the phenomena, the nervous path is interrupted, need not of necessity, as is generally asserted, be primarily situated in the posterior root zones of the spinal cord." The paper is illustrated by two well-executed plates.

J. M. P.

#### MITRAL STENOSIS.

CONstriction of the mitral orifice is, on many grounds, the most interesting of the valvular affections of the heart. It is common and at the same time dangerous, standing, in point of danger, next to aortic incompetence; being, indeed, the more serious of the two in early life, so that it has a very practical interest. Its clinical history, again, presents peculiarities, some of which have long been recognised, while others have not yet received adequate notice. The special claim of this condition, however, upon our attention, arises, as Dr. W. H. Broadbent, of London, points out in an article in the January number of the *International* (formerly the *American*) *Journal of the Medical Sciences*, from the fact that it presents greater difficulties in diagnosis than any other disease of the valves. It was the last of the valvular lesions to be associated with distinctive physical signs, and it is still not infrequently entirely overlooked by physicians, while more commonly it is diagnosticated as mitral incompetence, which is a far less serious affection. The physical signs are, in fact, extremely varied, and the attempt to elucidate their significance, and especially to attach diagnostic and prognostic meaning to some of the combinations of modified sounds and murmurs, is the chief object of his paper.

#### THE THEORY OF BRONCHIAL ASTHMA, VIEWED IN THE LIGHT OF THE PATHOLOGY OF HAY FEVER.

HAY fever occurs in winter or spring; sometimes it occurs at sea, or in the heart of a great city; sometimes, when no pollen can be found in the air, it arises after a full meal, or in the middle of the night; sometimes it appears almost instantaneously under the influence of intense light, the heat of a great fire, the odours emanating from certain localities, plants, and animals; some particular place or position occupied in driving, or from emotions and vivid ideas. In a suggestive paper in the January number of the *International Journal of the Medical Sciences*, Sir Andrew Clark points out that in these and in all like cases there are clearly two main factors at work, a certain local or constitutional predisposition, and some immediately acting exciting cause. That some such predisposition exists is plainly proved from the fact that the exciting agents which produce the malady in one class of persons entirely fail to produce it in



another; and that these exciting agents, in their relations to the persons acted upon by them, are in a remarkable manner specialised seems also proved by the circumstances that the emanations from a stable which in one person provoke a severe attack, produce in another, liable to hay fever, no sensible effect. And of the persons subject to this disease, it must be said that they are not always affected in the same manner by the same agent. When we inquire into the family and personal history of an individual subject to hay fever, we shall discover as the prominent point in it that the patient and his people are more or less "neurotic." There may be found among members of the patient's family the disease of which he is himself the subject, gout, such skin troubles as urticaria and eczema, migraine, neuralgia, epilepsy, and no inconsiderable sprinkling of pulmonary disease. But that which will be found the most widely, and will connect them all, will be a sensitive, an irritable, and an unstable nervous system. In a series of propositions Sir Andrew sets forth what he regards as the teaching of a study of hay fever concerning the pathology of bronchial asthma, holding that it is a neurovascular trophic disease, and has its roots in a special vulnerability of the respiratory mucous membrane, of the respiratory nerve centres, and of certain portions of the sympathetic.

#### FRIEDRICHSHALL WATER AS AN APERIENT AND DIURETIC.

DR. WHITLA, Physician to the Belfast Royal Hospital, in the newly-published third edition of his "Elements of Pharmacy, Materia Medica, and Therapeutics," says (p. 418):—"Of the various methods by which sulphate of magnesium may be used to produce its ordinary purgative effects, none equals the use of Friedrichshall bitter water. This valuable purgative, in addition to the sulphate, contains a large quantity of the chloride of magnesium, as well as chloride and sulphate of sodium, and other salts which act as mild stimulants to the peristaltic action of the bowel, so essential to the thorough evacuation of the entire canal. Friedrichshall water, by thus increasing both the secretion and the peristaltic action, produces a result in *chronic constipation* not unlike cascara in some respects; and the writer, by watching its effects, has found that the dose need not necessarily be increased, as is the case with most purgatives. It, moreover, acts upon the liver, and by keeping the skin warm or cold after its administration valuable *diaphoretic*, or *diuretic*, effects may be produced by small doses (2 oz.) diluted freely. If given fasting, its action upon the bowels is marked, whilst if administered after meals in even small quantities it increases the amount of the urine and the urea, and lessens the uric acid. Under the regular use of small doses (1 oz.) a surprising increase in the bulk of the *fæces* results, and the appetite becomes markedly increased. In *congestion of the portal system* the sulphate of magnesium, or Friedrichshall water, is the speediest and safest cathartic."

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OF

## MEDICAL SCIENCE.

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# THE DUBLIN JOURNAL OF MEDICAL SCIENCE.

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MARCH 1, 1886.

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## PART I. ORIGINAL COMMUNICATIONS.

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ART. X.—*On the Therapeutic Uses of the Digestive Ferments.\**  
By J. M. PURSER, M.D.

IF we may judge from the number of advertisements with which the journals are filled, and by the number of testimonials from chemists and practitioners which accompany these advertisements, there are few kinds of medicine more widely used at the present time than are the digestive ferments.

If we neglect the somewhat doubtful lactic acid ferment of the stomach, we know five digestive ferments:—1. A diastatic ferment found in the saliva and also in the pancreatic fluid; 2. A milk-curdling ferment found in the stomach and also in the pancreas; 3. A ferment which decomposes neutral fats, and which is found in the pancreas, and possibly in small quantity in the stomach; 4. A ferment which splits cane sugar into glycose and levulose, and which changes maltose into glycose—found only in the small intestine; and 5. A ferment, or, more properly, ferments, which convert proteid substances into peptone and other products. These ferments, which differ very much in their action, and in the conditions under which they act, are found in the stomach as pepsin, and in the pancreas as trypsin.

Now, as therapeutic agents there are some of these which we may eliminate, since their action is apparently unimportant for the process of digestion. We do not know why milk is curdled in the

\* Read in the Medical Section of the Academy of Medicine in Ireland, Friday, January 29, 1886. [For the discussion on this paper, see page 274.]

stomach, and I believe rennet is never given as medicine. The decomposition of neutral fats undoubtedly takes place in the intestine, but we have every reason to believe that this occurs only to a very limited extent, and that the absorption of neutral fats is effected for the most part without their having undergone any chemical change. Moreover, the fat-decomposing ferment of the pancreas is the most delicate of all the ferments, and even if it could be separated and administered it would be immediately destroyed by the acid of the stomach, as it cannot resist the action of an acid medium.

Thirdly, the inversive ferment of the small intestine is not much used in medicine, and we do not know to what extent in normal digestion splitting of the sugars of the saccharose group takes place. We have thus remaining only two ferments—the diastatic and the proteolytic.

The diastatic ferment is given either as preparations of malt or as pancreatic extracts, and there is no doubt that many of these substances do contain a ferment which changes starch to dextrin and sugar. But what is the value of this? In the body the transformation of starch takes place in the mouth and for a short time in the stomach. Then the action is stopped by the increasing acidity of the gastric juice and the salivary diastase is destroyed, not merely suspended in its action. In the intestine the action is again resumed and completed by the more powerful diastatic ferment of the pancreas, and usually there is very little unchanged starch to be found in the fæces. Now when diastatic ferment is given by the mouth it can only supplement the action of the saliva, for, before it can reach the intestine, it is destroyed by the acid of the stomach. The action of the saliva on starch is far less important than that of the pancreas, and, while after a starchy meal much unchanged starch is always found in the stomach, scarcely any is to be found in the intestine. In many of the lower animals who eat their food raw the saliva has scarcely any digestive action, and the starch is digested altogether by the pancreas. Hence the useful effect of diastase given by the mouth must be insignificant, even supposing that the preparation is good, and that the ferment in question is wanting in the saliva. But is this ever the case? I do not remember ever to have heard of an instance in which the physician found this to be so before he prescribed maltin or pancreas preparations; and I believe that the absence of ptyalin from the secretion of the mouth will be found to be

one of the rarest circumstances. I must therefore conclude that as regards diastatic ferments their administration as such is useless and irrational.

We now come to the proteolytic ferments, which are in many respects the most important of all. First, as regards the pancreas ferment or trypsin—it is generally stated that this ferment is destroyed by acid of the strength of that of the gastric juice, and there can be no doubt that this is true of many pancreas extracts. This being so, it is evident that it is useless to administer trypsin as a drug, for its activity will be destroyed if it be given while the stomach is digesting and contains acid, while if it be given during fasting, when the stomach contents are not acid, it is incredible that it could pass through the stomach and lie in wait in the duodenum until the next meal is taken, and, having undergone stomach digestion, has passed through the pylorus.

I have, however, found the statement that the proteolytic ferment of the pancreas is destroyed by acid to be not invariably true. I have lately worked with an example of the liquor pancreaticus made by Mottershead of Manchester, a very active preparation. This is acid, but what the nature of the acid is I cannot say. The liquor has a vinous smell, and the acid is possibly only that of the wine with which the preparation seems to be made. I have found that when the liquor was mixed with about five times its bulk of 0.2 per cent. hydrochloric acid and kept for twenty-four hours at 100° F. in the water bath, then neutralised and added to 1 per cent. solution of carbonate of sodium, it dissolved fibrin readily, although not so rapidly as before acidification. This solution was not due to putrefaction, as it took place when thymol was added to the digestive mixture, and when this lay for many days in the water bath it developed no putrefactive smell. Another statement which has been made by Kühne I have been also unable to confirm. He says that in a mixture of pepsin and trypsin the latter is destroyed, while the former is uninjured, and he suggests that in the body the bile plays an important part in precipitating and killing the pepsin before it can reach the pancreatic fluid. In order to test this I mixed equal volumes of Mottershead's liquor pancreaticus and liquor pepticus together, carefully neutralised the mixture with sodium carbonate, and left it for twenty-four hours in the water bath at 100°–104° F. I then divided the mixture into two equal parts; to one I added 0.2 per cent. HCl, to the other 1 per cent.  $\text{Na}_2\text{CO}_3$ , and placed fibrin in each. The alkaline fluid digested the fibrin readily; the acid even

after several days showed no signs of digestion. Here it would appear that the pepsin was destroyed, not the trypsin. Although I have repeated these experiments with constant result, I cannot affirm more than that what I have described took place with the particular material with which I worked, and I do not wish to throw any doubt on the contrary results got by other observers. To what the discrepancy is due, must be a matter for further investigation.

Still, even admitting that trypsin is not absolutely destroyed in the stomach, I think there is but little encouragement for its administration as a drug. Mixed up with the acid chyme such part of it as escaped absorption in the stomach would be precipitated by the bile along with the pepsin when the contents of the stomach passed into the duodenum, and even if it were again dissolved it would be so enfeebled in its action as to be of little use in digestion. But beyond this, is there any evidence that this ferment is wanting in the pancreatic fluid? Has any attempt ever been made to show this, or is there any known way by which it may be determined? It is more than probable that so long as pancreatic fluid continues to flow into the intestine it contains trypsin in far better condition than this ferment would be after having undergone stomach digestion. I would therefore condemn the administration of trypsin as I have done that of diastase.

We finally come to pepsin, and here, if anywhere, we have the ferment whose administration rests on a scientific basis. This ferment is easily prepared and kept, as it keeps well in an acid solution which is not prone to decompose. It can be introduced into the stomach without having undergone injury by the action of any other gland, and we can time its administration so as to introduce it when it is required. Is its administration, then, founded on sound principles? I believe not. In the first place, pepsin is very rarely absent from the gastric secretion.\* I have repeatedly examined matters ejected from the stomach. They were sometimes alkaline, or foetid—on one occasion smelling strongly of ammonia, but always when they were filtered and acidified they digested fibrin or albumen readily. It is, therefore, not to the want of pepsin that the failure of stomach digestion is usually due, but to some failure in the conditions under which only the pepsin will act. I do not deny, however, that there are cases in which pepsin is absent, and

\* This is in accordance with the large experience of Dr. C. A. Ewald. *Berliner klinische Wochenschrift*. 1886. P. 51.

that it may be possible to effect an artificial digestion in the stomach, but is that an experiment which any chemist would like to undertake?

If the condition of the stomach is so seriously affected as to cause a cessation in the secretion of the pepsin, it is most likely that the viscus is not a very favourable receptacle in which to carry on an artificial digestion. The matters introduced are withdrawn from observation and removed from control; we have no power of keeping them from hurtful mixture or of regulating the conditions so that they may be favourable to the process. In short, it seems to me that the stomach is about the worst place possible in which to perform a prolonged and delicate chemical experiment. Since, then, pepsin is very rarely absent from the stomach, and since, even in those cases where it is wanting, the conditions for its action are not favourable, I think that pepsin, like the other ferments, should be banished from the list of drugs employed for administration as internal medicine.

In what I have said I have supposed that the ferments in question are genuine, and capable, under favourable circumstances, of producing their digestive actions on the different alimentary substances. The pepsin preparations are usually good. As I have said, it is easy to make a good stomach extract. Many of the diastatic and pancreatic preparations are utterly worthless, while others are very good.

I suppose, too, that the different ferments are given with some regard to common sense, and not all mixed together regardless of whether they are mutually antagonistic, or whether they act in acid or alkaline solution. But even so, and considered as a question of physiological chemistry, I hold that these substances are not suitable for internal administration.

Of course it will be urged that everyone has seen cases in which the use of these ferments has been attended with marked success. To this I answer—firstly, that the treatment is never confined to the administration of the ferments, but that dietary and other precautions are adopted, to which more than to the medicine the improvement of the patient is due. Secondly, the fact that a patient takes a drug and gets better is no proof that the drug is the direct cause of the improvement. How many patients are cured yearly by homœopathic globules in the thirtieth dilution? how many patients are relieved of the most severe symptoms by having bits of metal fastened on their limbs, by breathing through a solenoid, or



by being put to bed with a horseshoe magnet? and yet does anyone really believe that these cures are produced by the treatment directly, and not through the mental effect the impressiveness of the treatment produces. When a patient gets a bottle of one of the digestive ferments he finds around it a sheaf of testimonials from the most eminent practitioners, and on the label of the bottle is a farrago of nonsense, which passes with him for science. He is told how these ferments (which, as every physiologist knows, have never been isolated or weighed) are here combined in the exact proportions in which they are found in the stomach; that he has here the means of digesting everything that can be swallowed; that he has in his hands a "medical certainty" which will cure all the disease the human frame is liable to. Is it any wonder that he feels comforted both in mind and body? Besides, these drugs have a great advantage over many others—they can do no harm. As placebos, then, I do not object to them, but do not let the medical man suppose that he is walking in the paths of physiological science when he prescribes them.

Are the ferments, then, of no use to the physician? On the contrary I believe them to be of great utility. The attempt to perform an artificial digestion in the living stomach I have deprecated; but the artificial digestion of food in clean vessels outside the body and the administration of the digested material is a mode of treatment scientific in itself, and one which is attended with the very best results. Here we have no interference of one ferment with another; no uncertainty of whether the ferment will find in the body the suitable conditions for its action. The whole process is carried on under our eyes and in circumstances which are under our control. This use of the ferments inaugurated by Liebig, and so much extended in late years, chiefly through the labours of Sir William Roberts, is I believe the true one, and destined to replace altogether their internal administration as drugs.

**ART. XI.—A Case of Spina Bifida treated by Excision.\*** By THOMAS SINCLAIR, M.D., M.R.C.S. Eng.; Demonstrator of Anatomy, Queen's College, Belfast; Surgeon to the Ulster Hospital for Children and Women, Fisherwick Place, Belfast; Assistant-Surgeon, Belfast Royal Hospital, &c.

**CASE.**—A plump, healthy, female child, aged three months, was brought to the Ulster Hospital for Children, on September 18th, 1885, and was admitted by my colleague, Dr. Poole, on account of a small spina bifida. She was free from any other congenital deformity, such as talipes, harelip, hydrocephalus, &c. The patient presented a small lumbar spina bifida, about the size and shape of half an egg, which the mother asserted was increasing in size, being certainly twice as large as at the time of birth. The following points in the anatomy of the tumour could be made out, and the consideration of some of these, with the mother's wish to have it removed, encouraged me to operate upon it in the way described hereafter:—Opposite the last dorsal and upper lumbar vertebræ a thin, translucent pellicle, beautifully injected with an open meshwork of blood-vessels, enclosed about half an ounce of clear fluid. Peripherally, where it was continuous with the perfectly formed skin, the edge of the latter could be felt very distinctly, as if a circular piece the size of a crown had been punched out of the cutis vera. A ring of thick, long, pigmented hair surrounded the junction, but the pellicle itself was hairless. There was a dimple near the summit of the tumour, the cause of which was determined at the time of operation. The floor of the cavity could be felt fairly, owing to the comparative flaccidity of the tumour; it was firm and tolerably even, except in the middle line, where two irregular hard points could be distinguished about one quarter of an inch apart, presumably two laminæ of an incomplete vertebra approaching each other. The dimple alluded to above seemed connected with the interval between the laminæ by a strand of tissue traversing the cavity. No alteration occurred in tension when the child cried; no head symptoms could be produced by prolonged pressure on the tumour. There never had been convulsions, nor other nervous symptoms of a paralytic nature. It had never burst. Nothing like large nerve cords could be felt or seen in the tumour.

On September 19th I tapped the tumour with a fine trocar and cannula under antiseptic spray, and drew off less than half an ounce of clear fluid containing a considerable quantity of albumen. All the fluid came away from one puncture showing a unilocular condition. The puncture was sealed by a piece of lint soaked in tinct. benzoini comp., a graduated compress of lint applied over it and strapped on, a firm

\* Read before the Ulster Medical Society, on Wednesday, December 2, 1885.

flannel roller applied over all. No unpleasant symptom occurred at the time. The child was rather restless during the first night. Forty-eight hours later, on removing the dressing, I found the tumour refilled, notwithstanding the compression. It contained more fluid than when tapped. The refilling seemed to indicate that the communication with the spinal canal was not quite obliterated; that it could not be free, the absence of increased tension on crying appeared to prove.

On September 21st I decided to excise the pellicle and convert the whole into a granulating ulcer, by stimulating the punched out edge of the cutis and the adjacent part of the base by some caustic. In the absence of anæsthesia, but with antiseptic precautions, I rapidly cut round the junction of pellicle and skin, but found that the former would not come away on account of the strong fibrous cord attaching the dimple near the summit to the base. Confident that it contained no nervous matter I cut through it. It was tough and ligamentous, but contained the largest vessels I encountered. The base, now exposed, was firm and smooth, except where the two bony points could be felt. No patent canal leading to the spinal canal was visible between these laminæ that was not completely filled by the stump of the strand of fibro-vascular tissue just described, and it did not appear prudent to probe between them lest some injury might be done. The bleeding stopped in a short time after holding lint, soaked in cold carbolic lotion, to the surface. The skin edges and the peripheral parts of the base were touched with nitrate of silver, a dressing of boric acid ointment covered by a piece of gutta-percha, rolled in lint and retained in position by strapping and a flannel roller.

No dangerous symptom occurred at or after the operation. The ulcer granulated kindly; later, exuberantly, and was dressed every alternate day with the ointment following a touch of the solid nitrate; the gutta-percha being retained till the last.

20th of October.—Within one month the healing was complete, and the patient was discharged. She has since been vaccinated and is thriving well.

*Anatomy.*—In studying the anatomy of a spina bifida it is well to consider the arachnoid, as divisible into anterior and posterior arachnoid, these terms being understood to mean the compartments in front of and behind the ligamentum denticulatum respectively.

The usual anatomy, according to Professor Humphry, of Cambridge, in a recent article in the *Journal of Anatomy and Physiology* for July, 1885, is “an accumulation of fluid in a cavity formed in the anterior subarachnoid, with all the structures posterior to it—viz., subarachnoid tissue, spinal cord and nerves, posterior

arachnoid membrane and cavity, dura mater, vertebral arches, subcutaneous and cutaneous tissues—stretched over it, and more or less imperfectly formed. It is evident from the above anatomical considerations that the failure of development upon which spina bifida depends must occur at an early period of foetal life—in most instances before the spinal cord has been segmented from the epiblastic, or epithelial, layer of the embryo from which it is developed.

“In some cases, however, the failure has occurred at a later period of foetal life, the segmentation and formative development of parts have taken place to a greater extent, and the skin, with its epithelium and the subcutaneous tissue, may have been formed in a natural manner all over the sac. This is most frequent in the neck.”

Again, Professor Humphry recognises that in some cases the segmentation and formation of the cord may be still more complete, so that it is found in the normal position on the bodies of the vertebræ. The dropsy in such cases, with the sac formed by it, is on the posterior aspect of the cord, the cord and nerves not passing into the sac. This condition is called by him “*hydrorachis externa posterior*.” The term “*hydrorachis externa anterior*” is applied to the more common variety, described previously. Both of these are to be distinguished from “*hydrorachis interna*,” in which the fluid accumulates in the central canal.

The fluid, in the majority of instances, is in the subarachnoid space in front of the cord. In some, however, it is in the arachnoid cavity in front of the cord, and then the sac is lined by the epithelium of the arachnoid. More rarely the fluid is found in the subarachnoid space or in the arachnoid cavity behind the cord. In these last cases the cord retains its normal position on the bodies of the vertebræ in the spinal canal; whereas, when the fluid is in front, whether arachnoidean or subarachnoidean, the cord and nerves are applied to, and probably blended with, the wall of the sac.

The anatomy of the tumour in my patient is peculiar in some respects, and probably warrants the placing of it in the rarer class of tumours, above alluded to, where the dropsy is in the compartment posterior to the cord.

Taking the central fibro-vascular strand, which traversed the cavity of the tumour as a pedicle, formed by the gradual approximation of the laminæ of the imperfect vertebræ, and which, from its connection between the bony points at the base, appears to be justifiable; and, taking the cyst wall as formed of the spinal membranes,

which the open-meshed arrangement of blood-vessels appears equally to warrant, it remains to account for the connection of the strand with the summit of the tumour. This may have taken place in one of two ways :—

1st. An adhesion may have occurred at some period of pressure between the summit of the tumour and the throat of the pedicle—i.e., the point where the pedicle passed between the ununited laminæ, thus leaving the sac of the tumour cut off from the arachnoid or subarachnoid cavity in the spinal canal—a condition which might also explain the absence of increased tension on crying.

2nd. The other explanation, which I think the less probable, may be offered:—A limited adhesion may have occurred at an early period between the parietal and visceral layers of arachnoid, at a point opposite the imperfect arch of the upper lumbar vertebra. Such adhesions are by no means uncommon in spina bifida cases, and are met with just below the lowest perfect vertebra, as described by Professor Humphry in some of his specimens. The subsequent distension of the arachnoid cavity with fluid, resulting in the protrusion backward through the imperfect arches of the dura mater and parietal arachnoid, would have the effect upon the visceral arachnoid of dragging it out of the spinal canal. The resistance of visceral arachnoid to this stretch would produce the very manifest dimple that existed at the summit of the tumour.

It is also possible that the later approximation of the laminæ so narrowed the basal part of the protrusion as to form a pedicle; and, had the patient come under observation some time later, the tumour might by that time have become completely detached from the spinal canal and its contents. As I found it, however, the throat of the pedicle had reached the stage of being blocked by the strand of visceral arachnoid. The two surfaces of arachnoid thus pressed into contact, adhered to each other at this part also, shutting off the cavity of the tumour from that in the spinal canal with which it was originally continuous. The difficulty in the way of accepting this second theory of formation is the presence of large blood-vessels in the strand of visceral arachnoid, seeing that this membrane has no proper vessels of its own.

Both theories as to the disposition of the parts show the cavity of the tumour to have been arachnoidean rather than subarachnoidean, and the fluid the over-secretion of the endothelium of that membrane. The fluid reproduced in the sac after my tapping was also of this nature, the albuminous character of the fluid pointing

strongly to the probability that the cavity was arachnoidean rather than subarachnoidean, for in subarachnoid fluid traces of albumen only, if any, exist. Mr. Holmes, in his "Surgical Diseases of Children," states—"Absence or even presence of a considerable quantity of albumen in the fluid is no proof that the tumour does not proceed from the spinal canal. In the latter case, however, it may be allowable to conjecture that its communication is with the arachnoid cavity, and that there is less probability of finding the cord in the sac."

*Ætiology.*—There are no facts in the history of this case tending to throw light upon the ætiology of spina bifida; nothing to enable us to decide between the two views held as to the origin of this condition—that is to say, whether spina bifida depends upon protrusion of the spinal membranes through a defect in the neural arches of the vertebræ, and in consequence of that defect; or whether the protrusion of the membranes, due to an intra-uterine arachnitis, is the cause of the defect in the arches. The child is a first-born, and gestation reached the full term. No accident happened to the mother during pregnancy.

Some interesting speculations have lately been published by Professor Cleland in the *Journal of Anatomy and Physiology*, as to the origin of spina bifida, anencephalus, &c., based upon the examination of a number of specimens of these deformities. He suggests that over-stimulation, at a very early period after conception, of the epiblastic elements—elements destined to form the cerebro-spinal axis—results in such an undue development of these parts, altering the usual shape and preventing the coming together of the medullary plates, by everting parts that ought to turn inwards. The over-stimulation is also expressed by a concurrent over-secretion of the epithelial cells, hence the dropsy. Over-stimulation is followed by a reactionary exhaustion, during which the development of the nerve-axis is suspended or delayed, and during which time it may undergo the atrophy and absorption of pressure produced by the fluid that itself secreted in the period of over-activity.

*Treatment.*—With respect to the treatment of spina bifida cases, most surgical writers, in warning against rashly interfering with them, emphasise, for the most part, those dangers immediately connected with the operation: the sudden fall of pressure in the central nervous organ, and consequent convulsions and death, or the occurrence of meningitis.

They draw attention to the frequent co-existence of congenital

hydrocephalus with congenital hydrorachis; but none of them suggest the possible *production* of hydrocephalus, in cases where it does not exist, by the spontaneous or artificial cure of a spina bifida.

Dr. E. Long Fox—in the *Bristol Medico-Chirurgical Journal* for March, 1885—has placed on record a case of spontaneous cure of spina bifida, *followed*, in some months, by hydrocephalus. In Dr. Fox's case, however, the central canal of the spinal cord was imperfectly closed, and the fluid which flowed during the first four months of life came from this central canal, as proved by the accumulation of nine pints in the cerebral ventricles subsequent to the closure of the open canal in the cord. There was a malformation in the lower part of the cord itself in his case, as verified by *post mortem* examination, which interfered with nature's efforts at spontaneous cure—in other words, this case was one of "hydrorachis interna."

As a prognosis is nearly all a surgeon can offer to many spina bifida patients, Dr. Fox's observations may, with advantage, be borne in mind. No symptoms of such a sequel have appeared in my patient up to the present; but, indeed, in this case one would scarcely anticipate such a result. It is now three months since the performance of the operation, and the patient is doing well.

#### ART. XII.—*The Poor Law Medical Charities System of Ireland.\**

By ARCHIBALD HAMILTON JACOB, M.D., F.R.C.S.I.; Ophthalmic Surgeon, Richmond Hospital; Professor of Ophthalmic Surgery, Royal College of Surgeons in Ireland.

GENTLEMEN,—This time last year, Dr. John William Moore, whose then position as Chairman of this Sub-section I have the honour to occupy this evening, addressed you on the subject of "Sanitary Organisation in Ireland in its Medical Aspect," and laid before you a lucid narrative of the causes which produced the breakdown of the Public Health System in Ireland, and the disastrous result of that breakdown upon the health and lives of the people, and upon the prosperity of the country. I believe that I can scarcely occupy your time this evening to greater advantage than to supplement Dr. Moore's observations, by submitting to you a sketch of the Poor Law Medical Service of our country, and by offering for your consideration a few suggestions for its improvement in detail. I feel that as I have never held office myself, as

\* An Inaugural Address delivered before the Sub-section of State Medicine of the Academy of Medicine in Ireland, Thursday, February 4, 1886.

either a dispensary or workhouse medical officer, I am more or less open to the complaint that I am intruding as an amateur into the arena more fittingly occupied by those who have acquired personal experience of the working of the Medical Charities System. But I venture to plead that as I have, for the last twenty years, been, in some sense, the depository of the confidences of my brethren who administer that system, I may claim to have thus acquired a knowledge of the minutiae of the system, and of its weak points, which knowledge I may advantageously make use of to inculcate reforms.

The working of the Poor Law Medical Charities System in Ireland, is indeed a subject well deserving of the attention of this Sub-section of the Academy; for it occupies the closest relation to the welfare of the most helpless portion of the population of Ireland, and to the prosperity and producing power of the country, and, moreover, affects the closest interests of the greater number of our own profession throughout the country.

Including in the designation of "Poor Law Medical Charities," the Workhouse Medical Relief System, I find that it affords medical aid annually to over 840,000 persons. or nearly one-fifth of the entire population of the island; of whom over 640,000 may be described as sick when brought within the scope of the system. But in addition to the relief of the indigent sick, which is their special function, the Poor Law Medical Officers of Ireland have the medical charge of the Constabulary, numbering at present 12,938; their families, numbering, on a calculation of two persons to each family, nearly 26,000; and the Coast Guards, numbering about 1,500. Furthermore, the vaccination of 117,484 children and adults, calculating the average of three years, 1881-3, is effected annually by them. And lastly, they examine and certify about 1,200 dangerous lunatics.

So far I have enumerated the medical and curative functions which Poor Law Medical Officers discharge for the public; but they have other duties of scarcely less importance, which, of necessity, bring them into intimate communication with the population, under conditions which enable them to exercise most valuable influences upon the welfare of the people. In the registration of an annual average of 126,014 births, they are brought into contact with the people at the moment when their advice and aid may yield the most valuable result upon the health of the community. In their function of Registrars of the 96,552 deaths which occur :-



Ireland in each year, they become directly and immediately cognisant of the perturbations of the public health throughout the country, and the invasions of epidemic and infective disease, and are enabled to grapple with these enemies to public prosperity at the instant when a resistance to such hostile influences may be most effectually undertaken. Lastly, in being called upon to register an annual average of 23,375 marriages, they acquire a personal knowledge of the domestic life of the people, and often are accepted as their confidential advisers, not only in matters of health, but in the other affairs of life.

Their duties as Medical Officers of Health—which have been the subject of the address of my predecessor—would be, if they were permitted by the Local Government Board and by the Union Guardians to perform them efficiently, amongst their most important functions, although they do not permit of being included in the foregoing classification, because, in their capacity of Medical Sanitary Officers, they are responsible for the detection and annihilation of every nidus of disease, and are responsible to advise the people as to the maintenance of their domestic health, and, if necessary, to enforce protective measures.

Finally, the examination and certification of young persons employed in factories—though not within their jurisdiction as Poor Law Officers—is, in most instances, performed by them, and is of sufficient public importance.

These figures I may present in a more intelligible form in the following table, which sets forth the numbers of the Irish population ministered to annually by the Poor Law Medical Charities Service:—

Admitted to workhouses, sick	-	-	53,105
Attended on Medical Relief tickets	-	-	588,553
Total sick	-	-	641,658
Healthy when admitted to workhouses	-	-	200,237
Total under Poor Law Medical			
Charity	-	-	841,895
Constabulary and Families, say	-	-	38,814
Coast Guards, say	-	-	1,500
Total population in Medical charge			
of the Poor Law Service	-	-	882,209
Or 17·7 per cent. of the entire population.			

**ADDITIONAL DUTIES—**

Vaccinations	-	-	-	117,484
Birth Régistrations	-	-	-	126,014
Death do.	-	-	-	96,552
Marriage do.	-	-	-	23,375
Certification of Lunatics	-	-	-	1,200
				<hr/>
Total	-	-	-	364,625

Thus it appears that the Poor Law Medical Charities Service of Ireland passes through its hands, in its proper medical or curative capacity, in the course of the year, nearly one million persons, or one-fifth of the entire population; and, in addition, is called upon to intervene in 365,000 separate occasions, by which the health, prosperity, and good order of the people is most closely influenced. Thus, allowing for the re-counting of persons who come more than once in the year within the scope of the system, I am entitled to assert the fact, that a million and a half persons, or one in three, of the entire population pass through the hands of the Irish Medical Charities system in the course of each year.

To view the extent and importance of the service, from another point of view, we may regard the amount of public money which is expended for the maintenance of the health and prosperity of Ireland through the agency of the Poor Law Medical Service. In this connection I may shortly recapitulate as follows the items of yearly expenditure for which the dispensary and workhouse medical officers were most directly responsible, in the year 1884 :—

Dispensary and Vaccination Services,	-	£158,363
Registration of Births and Deaths,	-	10,954
Sanitation,	-	58,844
Salaries, &c., of Workhouse Medical Officers,		8,935
Medicines, Medical Appliances, and Sick		
Nurses, say	-	10,000
Superannuations,	-	8,364
		<hr/>
Total,	-	£255,460

Furthermore, the Poor Law Medical Service is very widely distributed and is numerically very extensive, and forms unquestionably the most important section of our profession in Ireland. Its numerical strength at present is as follows :—

Workhouse Medical Officers and Apothecaries,	-	202
Dispensary Medical Officers and Apothecaries,	-	849
Midwives,	-	296
		<hr/>
		1,347

I find, from an examination of the "Irish Medical Directory," that the members of the medical profession resident at any one time in Ireland number about 2,200; but, allowing deductions for those who have retired from practice or who (being connected with the army, naval, or commercial services) are peripatetic, I calculate that the actual working members of the profession do not exceed 1,800. If, therefore, 1,051 of these (as above shown) are connected with the Poor Law Medical Service, I will not be accused of unduly exalting their importance in speaking of them, as I have done, as "unquestionably the most important section of our profession;" and having before me the figures above quoted, I do not hesitate to go further than this in asserting that the Poor Law medical officers of Ireland are incomparably the most important public servants of the community, and that their functions are of immeasurable value as exercising the most direct and lasting influence upon the prosperity and happiness of the Irish people.

I apprehend that these facts are scarcely appreciated either by the people themselves or by our rulers either in Parliament, the Irish Government, the Local Government Board, or the Boards of Guardians; for the enactment of the medical poor law and its administration seem to be conducted in the most haphazard and perfunctory manner. The Poor Law Medical system is liable at any moment to be disturbed by legislative changes and authoritative rules, to which neither Members of Parliament nor any one else give due thought. New duties are thrust upon the service without inquiry or serious thought as to its fitness for the charge; they are expected to perform these duties without any approach to adequate instruction on the subject and without any effort to organise, and they are called upon to discharge these new functions under the guidance and control of Guardians who are themselves absolutely ignorant of their own powers and responsibilities in the matter, and totally uninstructed on the method of carrying the law into effect.

I need only point to the history of the enactment and enforcement of the Labourers Dwellings Act of 1884 as an illustration of my meaning. A Bill, conceived in the last month of the session,

was rushed through both Houses, without even a pretence of revision, at the moment when half the M.P.'s had gone off to the grouse shooting, and the remainder were utterly *blasé* of legislative work. It was to come into operation in the following winter, and three months was given to the Guardians to take action under it; yet, when the winter arrived, the minds of both the Guardians and their officers were an absolute blank on the subject. A condition of chaos, which would be ludicrous if its effects were not so disastrous, was the result—every mistake was committed which ignorance of the business could beget—the clauses of the Act were infringed by every Board of Guardians which attempted to make use of it; and the result has been universal wrangling, costly litigation, the break-down of the Labourers Dwellings system, and numerous past, present, and future attempts to tinker-up the Act.

The Irish Poor Law Medical Officer is presumably a gentleman of culture and competent skill, for not only must he possess the diplomas in Medicine and Surgery which are required for the Army and Naval Services, but he must also be specially qualified in Midwifery, and these imply an amount of general education and culture which pass muster. He is appointed—if a Workhouse Officer—by the Guardians, under the 31st section of the Poor Relief Act of 1838 (1 and 2 Vict., cap. 56); if a Dispensary Officer—by the Dispensary Committee, under the 8th section of the Medical Charities Act of 1851 (14 and 15 Vict., cap. 68). In the case of the Workhouse Officer the Local Government Board is required by the first quoted section to “direct the mode of the appointment, and determine the continuance in office or dismissal of such officer,” and by their own rule (Art. 39), made under authority of that section, they order that—“Every Officer appointed to or holding any office . . . shall, . . . subject to the provisions of Article 40, continue to hold the same until he die, resign, or be removed by the Local Government Board.” This power of removal of Officers by the Local Government Board is defined by the 33rd section of the Poor Relief Act, which says:—“The Commissioners may . . . either upon or without any suggestion or complaint in that behalf from the Guardians, . . . remove any paid officer appointed under the provisions of this Act whom they shall deem unfit for or incompetent to discharge the duties of any such office, or who shall at any time refuse or neglect to obey and carry into effect any of the orders of the Commissioners.”

The Dispensary Medical Officer holds his office subject to the one brief proviso in the 8th section of the "Medical Charities Act," that "it shall be lawful for the said Commissioners to remove any such medical officer on sufficient grounds." No further provision exists.

The question of the appointment and tenure of Poor Law Officers raises the great question of their status as public servants, and is worthy of the deepest consideration. From the law as I have quoted it above it is obvious that the original intention was that while the local authorities, being the paymasters, ought to please themselves as to the officer whom they might select to serve them; that officer, when appointed, should be essentially a civil servant, holding his position during good behaviour and independently of the local authority.

It seems to have been fully understood by the law-makers of that day that the residents of a district, who were taxed for the maintenance of the doctor, had every just right to select the officer who was to serve them as long as that officer was fully competent for his duties; but it was also fully understood that such officer could not efficiently discharge his onerous and often invidious functions unless he were placed under the immediate protection of a central authority and in a position of safety and independence as regards his tenure of office.

I regret to say that these wise principles and opinions have to a great extent fallen into disuse, and that the service is in danger of becoming the menial servant of the local authorities, wholly dependent on their caprices for daily bread, unprotected in the discharge of its duties, and therefore necessarily subservient and inefficient in all matters in which those duties touch the personal interests of Guardians and Committee-men. In the first place, the power of appointing the officers, which is confided to the local authorities, is, in very many instances, so exercised that the most competent officer is not necessarily selected, for it is notorious that in almost all parts of Ireland politico-religious qualifications rule the elections, while scientific attainments, experience, personal qualities, or social culture, weigh but little in the selection of a candidate.

It is, no doubt, reasonable that the taxpayers of a district should lean towards a competitor with whose religion and politics they are in sympathy, but that consideration should never be allowed to overshadow the interests of the sick poor, or to cause an officer of inferior capacity to be placed in charge of them; and yet it

is notorious, that frequently the professional merit of the candidate is put completely out of sight, and political influence is substituted to such an extent, that it is not very unusual to see elections conducted in the presence of riotous political mobs, under the loaded rifles of the police, and amidst all the intimidation of a contested parliamentary election. Such a state of things cannot fail to react disastrously, and I think it has so reacted, upon the status of the Poor Law Medical Service, and against the interest of the sick poor, and yet it is difficult to suggest a remedy. It has frequently been suggested that the entry to the service should be by competition, and that officers should be promoted from one district to another by their seniority as a *roster*, but I cannot look upon such a proposition as just, even in theory, inasmuch as it would deprive the residents of a district of all voice in the selection of their medical attendant, while it is obvious that the tide of public opinion at present is directly against such a centralising scheme, and in favour of concentrating in the local authority an almost uncontrolled discretion as to the administration of its district.

While, however, I recognise the unwisdom of any undue interference with that discretion—even though it may be somewhat abused—I regret to observe that the Local Government Board has displayed in the matter of Poor Law appointments the same indifference and the same desire to escape responsibility by which its administrative policy has always been characterised, for I have never known of an instance in which its influence has been exercised to secure fairness of election or appointment by professional merit. It would have been open to that Board, I think, to put a stop to political intimidation, when that intimidation went the length of obstructing and coercing the voters and maltreating the unpopular candidate; and, I think, they would have been quite justified in refusing their sanction to elections so conducted, but on no single occasion have they ventured even to advise the electors, still less to protect the voters, or secure the freedom of their choice.

The remuneration of the Poor Law Medical Officer must, I think, always be governed, in the greatest degree, by the ordinary principles of demand and supply, for though I am aware that Guardians and Dispensary Committee-men are often impelled under pressure of the local taxpayers to select their officer from the “cheap and nasty” grade of the profession, yet I do not think that, on the whole, the interests of the sick poor have been materially prejudiced by this very natural leaning towards economy.

In fact, in most districts the amount of the official emoluments is not the chief factor which decides the class of doctor to whom the district is to be entrusted; much more important considerations are the extent and value of the private practice obtainable, the nature and geographical position of the district, and the chance of agreeable social intercourse therein. A poor district, sparsely inhabited by indigent tenant-farmers, and far distant from a large town or from a railway station, must be content with the ministrations of such a medical officer as will be content to live in it, and who will compensate for the absence of practice by a little farming; and it would be neither possible nor desirable to urge the local authorities to pay their doctor anything more than the minimum for which a duly qualified practitioner can be obtained; nor would the addition of a few pounds to his salary purchase the services of any medical officer whose attainments and tastes enabled him to live elsewhere.

I have, therefore, always discouraged agitations for the raising of salaries; and I recognise the fact that neither the Local Government Board nor any other supervising authority ought to dictate to the Guardians on this question. But it is, nevertheless, clearly the duty of that Board—and I am glad to say they have hitherto been firm on the subject—to protect the officer against capricious and malicious reductions in his salary when once fixed; for if it were permitted to the Guardians to deprive him of a portion of his means of livelihood whenever the economic shoe pinched them, or whenever they saw fit to punish him for some offence against duty or political creed, it is obvious that the emoluments of the Poor Law Medical Officer would come to be so precarious, that appointments would cease to be an object of competition.

But, as I have said, the value of a Poor Law medical appointment, and the quality of the doctor who seeks it, are dependent chiefly on the value of the private practice obtainable in the district; and this brings me to the discussion of the cause which influences that value in different localities. As you are all aware, the dispensary doctor is required by the 9th section of the Medical Charities Act, and by the 21st article of the Regulations made thereunder, to “duly and punctually attend, either at the dispensary during the appointed hours for attendance thereat, or at the house of the party on whose behalf application is made, or elsewhere, as the case may require, and supply all requisite medical and surgical advice and assistance to every *poor* person in the district or division of a district under his charge, whom he shall be required to attend as medical officer,

by a ticket, signed by a member of the committee of management of the dispensary district, or by a relieving officer or warden, acting for any electoral division included in such district; and shall continue his attendance until such attendance is no longer required, or until the ticket be cancelled."

This law was made in the year 1851, shortly after the famine, and at a time when it was peremptorily necessary to provide for the medical care of the poor in the widest sense; but within the thirty-four years which have since passed, though the condition of the Irish people has completely changed, the law has remained unaltered either by the voice of Parliament or by the authority of the Local Government Board. That Board has steadily refused to define the word "poor;" has permitted (I might almost say encouraged) the irregular issue of medical relief tickets to persons who could not pretend to be poor in any true sense; and has, in every way, lent its official toleration to the wholesale abuse of the system. As a natural consequence, the Poor Law medical relief system of Ireland has become wholly demoralised—a large section of the population, who in any other country in the world would never dream of seeking medical charity, have quartered themselves upon the taxpayer, and, unblushingly, demand gratuitous medical advice and medicine. To such an extent has this demoralisation extended in Ireland that in many districts the small farmer, petty shopkeeper, or well-to-do artisan, stands aghast at any suggestion that he should pay for doctors or medicine; and in some districts, which are by no means amongst the poorest, practically the entire population claim to be regarded as paupers *quâ* medical attendance.

I need scarcely remind you that such a malversation of medical charity does not exist in any country under the sun. In England, Wales, and Scotland, no one is entitled to medical poor relief unless he is a pauper; and it is the duty of the relieving officer—who alone is authorised to issue medical relief tickets—to satisfy himself that the recipient is actually destitute before he issues it. No artisan who is well enough to work is regarded as a poor person. In Ireland, on the other hand, medical relief tickets may be issued by any one of, say, ten committee-men and wardens, and very generally no inquiry is made as to the capacity of the recipient to pay. If any consideration at all is given to that question, the issuer proceeds on the principle that no one should be called upon to pay for medical advice and medicine unless he has loose money for the purpose over and above all his other requirements; and, in fact, if



it seems that an applicant spends his daily earnings on either necessities or luxuries, he is granted a medical poor relief ticket as a matter of course. It is no uncommon proceeding for the committee-man to sponge his own family on medical charity; and, in doing so, it never occurs to him that there is anything unlawful or opprobrious in doing so, for, by long experience, he has been taught to believe that the services of the dispensary doctor are included in his taxes, and that he is as much entitled to use those services as to use the roads which he helps to pay for.

If such person hesitates at all about issuing a ticket, he does so because of personal friendship for the doctor and a desire to spare him trouble.

I do not appeal against these abuses in the interest of the doctors, for, in the long run, it does not make much difference to the profession at large how the system is worked. It is manifest that some one must pay them for their services, and that the sum they receive must be enough to live on. If they derive a considerable income from private practice they can afford to accept a lower salary from the Guardians, but if they are excluded from private practice by the operation of these abuses they must either insist upon a larger salary or else seek their fortune in a more lucrative locality. Thus in districts where these abuses are rampant, the taxpayers have to pay inordinately high salaries, and find the greatest difficulty in obtaining or retaining efficient officers, and I have in my mind's eye certain places in which, for this reason, no medical officer remains for more than a few months. Furthermore, the taxpayer is called upon to pay for the medicines and medical appliances supplied to well-to-do people, and, as a result, the expenditure on these items throughout Ireland is out of all proportion to the amounts paid in other countries for a similar population.

The abuse of Poor Law medical relief has been fostered by one or two flaws in the law, and, as I have said, by the indifference of the central authority—the Local Government Board. I have already complained that no attempt has been made to define a “poor” person, such definition being the more necessary because the medical officer cannot be permitted to make such a definition for himself. I fully concur in the view that the dispensary doctor should be divested of all discretion in the matter, and that he should be, as he is, bound to attend on the ticket no matter by whom it is presented, but I look upon it as monstrous that the supervising authority should tolerate the indiscriminate issue of tickets as they do. Under

their *regime* the book of dispensary tickets has, in many districts, come to be an important part of the stock-in-trade of the local shop-keeper, who, in order to be in a position to oblige his customers, obtains appointment as a committee-man or warden, and, having obtained his book of tickets, signs his name at the bottom of each and hands them over to his shop-assistant for issue to every customer who desires one. The order of the Local Government Board is that "the ticket and counterpart are both to be filled up by the person authorised to issue" it, the purpose of such direction being obviously that the issuer may satisfy himself that the recipient is deserving; but the Local Government Board has allowed this rule to become a dead letter, and has even, when called upon to give an opinion upon this system, refused to discourage it in any way. It is a question open to legal dispute whether a ticket, not filled up by the issuer, is binding on the medical officer; but, practically, that officer is obliged to honour it because of the difficulty of proving that it was issued by deputy, and because of the risk of censure or dismissal if he refused to comply with the order.

These abuses of Poor Law medical relief are also facilitated and encouraged by the fact that the remedy which the law places at the disposal of the medical officer is, in all respects, unsatisfactory. If he should feel aggrieved by the improper issue of a ticket, his only redress is to seek to have it cancelled, for which purpose he must resort to the dispensary committee—where he is confronted with the member who is in fault and by his friends—it may be, perhaps, several weeks before the meeting of the committee takes place, and, meanwhile, he must continue attendance on the patient whose case, probably, will be disposed of long before the redress sought for can be achieved. If he thinks it worth his while to press for the cancelling of the ticket, the committee, desiring not to condemn their fellow-member, may probably decide that the ticket was rightfully issued; but, if favourable, they may cancel it, in which case the committee-man who issued it may revenge himself by at once issuing another ticket for the same patient, upon which the medical officer is still bound to attend. Thus great injustice and hardship may be inflicted on the medical officer even by inadvertence, but those hardships are multiplied tenfold where the wholesale issue of tickets is—as it often is—made use of by a malicious member of the committee to persecute a medical officer to whom, for political, religious, or personal reasons, he may be hostile. It is quite open to him in such case—and the instances are not rare in which it has been done—to

render the doctor's life miserable and to destroy his professional prospects by maliciously serving him with visiting tickets. I have known cases, and I have one under notice at the present time, in which the hostile committee-man has hunted the country for persons to whom to issue tickets, has designedly kept the presentation of those tickets back until a time when it is most harassing on the doctor to attend on them, and has made it a rule to give visiting tickets for long journeys to patients suffering from the most insignificant maladies.

To remedy these abuses and counteract these faults in the law, the Irish Medical Association has done much. In the first place, it has taught the committee-men that if they negligently or wilfully issue tickets to persons who have no right to them, they may themselves be sued for the doctor's fees, and made to pay; secondly, it has taught the person who asks for a ticket, without being entitled to it, that he may be punished for the false pretence by being compelled to pay. Upon these points the Association has obtained legal decisions, and has thus put the weapon of defence into the hands of the medical officer, but it is not yet clearly established whether the cancelling of the ticket is a necessary preliminary to the suit, and, as it has been assumed to be so, the medical officer remains, in most instances, helpless until some means is found of speedily cancelling improperly issued tickets. A method of doing this, which seems both reasonable and practical, was embodied some years ago by the Irish Medical Association in a bill introduced on their behalf by Mr. Meldon, then M.P. It proposed to give to the magistrates at petty sessions a power to cancel tickets on good cause shown, while still reserving the same power to the dispensary committee. This clause has not been pressed, nor do the present circumstances encourage the Association to submit it again for the approval of Parliament, but I think it an admirable proposal. As the petty sessions meet, as a rule, once or twice weekly, an appeal to the magistrates might be taken at the beginning of one attendance, and the appeal tribunal would be quite independent in its judgment, while it would be in a position to adjudicate upon the merits of the dispute with a certain amount of personal knowledge.

It seems to me that if such a check were placed upon the committee-man, warden, and patient, very few of the existing abuses would survive, and the interests of the sick poor and of the taxpayer would be greatly served.

The outcry of the Poor Law medical officers against these abuses

has been met by the statement that there is no abuse; that the tenant farmers, petty shopkeepers, and artisans, if not actually "poor" persons are relatively "poor," because they are quite unable to pay the guinea which is the recognised doctor's fee; that it is, therefore, the medical officers' own fault if, by keeping their fees above a practicable amount, they force the committee-men to issue a ticket to every one who cannot afford to pay the guinea. This answer to our complaint is enforced by pointing out that in England, Wales, and Scotland, none but the destitute are beyond the reach of private medical advice and medicine, which may be obtained from fully qualified and reputable practitioners through the agency of clubs and provident dispensaries for a payment of a few pence weekly or a few shillings a visit.

The Irish Poor Law medical officers are told to "go and do likewise."

Now, in the first place, it is quite untrue that the Irish Poor Law medical officer either demands or receives a guinea a visit, or that his ministrations are, on the whole, anything more costly than those of his Saxon brother. It is true that some of the seniors in the service adhere to the symmetrical guinea as their recognised *honorarium*, payable at a first attendance on the patient, but the repetition of that fee is entirely dependent on the monetary capacity of the payer. Having, as they consider, preserved their self-respect by receiving the fee which recognises their status as gentlemen and educated physicians, they are quite willing to give any quantity of gratuitous attendance which the patient has any right to expect, and I believe I am correct in saying that the whole amount which they receive for each private patient is much less than that which the same patient, if resident in England, would have to pay either in club subscriptions or private fees for the same services. I assert, without fear of contradiction, that in any case which requires several visits the Irish patient is enabled, without resorting to the relief ticket, to obtain medical advice upon much easier terms than in England, even though he may have, at the first visit, to pay his guinea to the doctor.

But then there are the patients who could pay a few pence weekly (if they would) or a half-crown at each visit, but who are so indigent or improvident as not to possess a guinea at the moment when the doctor's services are required, and it cannot be denied that these are left unprovided for by the guinea-fee system, and are, of necessity, thrown upon the medical charity as "so-called poor

persons," and I think, therefore, that some relaxation of the recognised *honorarium* ought to be made.

It is obvious that to attempt by etiquette or understanding to impose strictly on the practitioner any fee or scale of fees would be not only unbusiness-like and impracticable but unwise and unreasonable, and therefore the attempts occasionally made by Boards of Guardians to pledge the medical officer to accept a fixed small fee are the stupid outcome of ignorance of the ordinary principles of political economy. Not only the necessities of the patient and his capacity to pay the doctor vary in all degrees, but the value of the doctor's skill and time are equally unfixable. Many Poor Law medical officers are well worth their guinea fee, and are so over-employed that their time would be underpaid by any smaller sum; while others, perhaps, would be considered by their patients to be dear at half-a-crown. It is, therefore, impossible to fix a minimal fee; but, on the other hand, it is time, I think, that the attempt to fix a maximum by any rule of medical etiquette should be abandoned. I hold that every practitioner should be permitted to assess the value of his own services without being banned thereby as having fallen from medical virtue.

As a matter of fact, I believe a majority of the Poor Law medical officers of Ireland have taken the matter into their own hands, and, while they maintain the theory that their proper fee is a guinea, they are quite prepared to accept half or quarter that sum without saying anything about it; but there are many others who feel themselves bound by medical propriety to adhere to the fee which their ancestors required, and I think it fair and reasonable, in the interests of the sick poor and of the profession, that those practitioners who wish to adjust their fees to the needs of their clients should be by some means released from this supposed obligation. With this object I suggest that the Irish Medical Association, or some other professional organisation of equal influence, should adopt and formally promulgate a resolution to the effect that a medical practitioner receiving such fee as the circumstances of his patient enabled him to pay should not be considered to have infringed professional propriety.

But though I contend for the right and propriety under the circumstances of each practitioner fixing his own fee, I recognise the necessity for establishing, as far as possible, a uniformity of action in the matter amongst Poor Law medical officers, and with this view I think it would be very desirable if a scale of fees could be suggested. Such scale should be clearly defined to be a minimum,

inasmuch as the right of the practitioner to exceed it to any extent he pleased ought to be fully declared; and I do not see any fairer basis upon which such a scale could be constructed than the valuation of the patient's residence. Such a tariff of fees has been in use for years in many parts of England, and it has, I believe, been found to work satisfactorily, but it may be that in the construction of such a scale for the Irish provinces the distance travelled by the doctor in his visit to the patient should be taken into account.

It seems to me the voluntary adoption of such a tariff in any district would do much to neutralise the abuse of medical charity relief therein without any change in the existing law. It would be open to the medical officer to supply to the committee-men or wardens a book of "recommendations," which would serve not as an *order* to the doctor, but as a recommendation to him to give the holder the benefit of the reduced scale of fees. It would be thus open to the committee-man, when importuned for a medical relief ticket by an undeserving applicant, to afford him that relief without burthening the doctor with gratuitous work or mulcting the rate-payers for cost of medicines, and I think it would be found to be the interest of the medical officer to encourage the committee-man to substitute these recommendations for the official medical relief ticket by allowing the issuer a considerable discretion as to their issue.

#### THE POWER TO DISMISS THE POOR LAW MEDICAL OFFICERS.

The question which now most fiercely burns in the Poor Law medical world is the tenure of office of the workhouse and dispensary medical officer. In the case of the dispensary officer, as I have already pointed out, the only law on the subject which exists is that which enables the Local Government Board to dismiss an officer "on sufficient cause." Heretofore that power has been exercised rarely and in cases where there was obviously good reason, so that no occasion has arisen to take a legal decision as to what constitutes "sufficient cause;" but I think it may be taken as the law that a dispensary medical officer holds his appointment until some circumstances arise which a jury would consider to justify the Local Government Board in dismissing him. It is possible that the question may come to be discussed whether the hostility of a majority of the dispensary committee, either on political, religious, or personal grounds, could be regarded by the Local Government Board as "sufficient cause," and if such dispute arises I have no doubt that the Irish Medical Association will see the propriety of asserting,

on the part of the dispensary officers, the principle that, as long as they are competent and willing for the efficient discharge of their duties, their tenure of office is permanent.

In the case of the workhouse medical officer the question of tenure of office is now ripe for settlement, and will be actually under argument in the High Court of Justice within a day or two. The right of the guardians to dismiss an officer was first broached in 1881, in the case of Dr. O'Reilly, of Lismore, whom the Local Government Board encouraged the Guardians to dismiss. They were met by the Irish Medical Association with the challenge that if the Guardians took any such course an action would be at once instituted by them on Dr. O'Reilly's behalf, because, by law, the dismissal power is vested in the Local Government Board, and cannot be transferred to the Guardians by any other means than a new Act of Parliament. When the case was ripe for contest, Dr. O'Reilly, against the advice of the Association, threw up the sponge and resigned his office, and since then the Association has been watching for the opportunity which now presents itself to settle the dispute.

In 1881 both the law and their own regulations were clearly against the Local Government Board in their effort to shift the dismissal responsibility to Guardians, but, with the view of mending the law, and to give legal effect to their views, that Board promulgated, on the 18th of December, 1882, a new general order for the administration of unions. By the pre-existing rule the Guardians might "suspend" an officer pending the approval of the Local Government Board. If that Board did *not* approve, they might restore the officer to office, otherwise they must take the responsibility of dismissing him themselves. By the new rule the Board try to relieve themselves of this responsibility, for they decree that, if they do not remove the suspension, then the Guardians may dismiss him. There is, fortunately for the Poor Law officers, one flaw in this nice little scheme to throw them on the tender mercies of local political agitators. The Local Government Board has no more power to make such rules than to dictate orders to the Lord Chancellor. The Act of Parliament distinctly invalidates any or every attempt to shift the responsibility of dismissal of its officers from that Board to any person or body whatever. There is but one Act of Parliament which governs the matter; it runs thus:—

"It shall be lawful for the Commissioners . . . to direct the Guardians . . . to appoint such paid officers as the Commissioners shall think necessary . . . and the Commissioners

may . . . *direct the mode of the appointment and determine the continuance in office or the dismissal of such officers*, and the amount and nature of the security, &c. . . .”

It will be noted that under this section the Commissioners, and no one else, are to “*determine the continuance in office or the dismissal of such officers.*” These words of themselves might be considered sufficiently explicit, but they are confirmed by the phraseology of the 33rd section, and by the Order originally made by the Commissioners themselves, and now, with singular inconsistency, reiterated. The 33rd section says:—

“The Commissioners may, *either upon or without any suggestion or complaint on that behalf from the Guardians of any Union to remove any paid officer . . . whom they shall deem unfit for or incompetent to discharge the duties of any such office.*”

It is here set down, one would think, sufficiently clearly that the Local Government Board are the persons to dismiss, and that the Guardians have no power in the matter save to offer “suggestions or complaint,” to which the Board may or may not give effect; and it is to be noted that the officer is liable to dismissal only upon incompetency or unfitness, or for refusal to obey *the orders of the Commissioners*—it being clearly the intent of Parliament that the officer should not be open to removal because of declining to fulfil the Guardians’ orders, or to conform to their personal caprice, and that he should be, to that extent, independent in the discharge of his duties.

Neither in this clause, nor in any other part of any Act of Parliament that I can find, is there any power whatever given to the Board to delegate dismissal powers to any person or body; and, indeed, such delegation is contrary to the whole spirit of Irish Poor Law legislation. The Local Government Board themselves interpreted the law in this sense, for, acting upon the authority of the law which we have quoted, they have made the following rule:—

“ARTICLE 39.—Every officer . . . holding any office under this Order shall . . . continue to hold the same until he die, or resign, *or be removed by the Local Government Board*, and every porter or assistant may be dismissed by the Board of Guardians without the consent of the Local Government Board.”

It will be noted that herein a marked distinction is drawn between those menial servants which the Guardians might dismiss, and those higher officers which the Local Government Board alone was entitled to deal with; and nothing can be clearer than that that Board, and they alone, have power to remove the higher officers.



The Irish Medical Association has taken the highest legal advice on the point, and has received from Mr. Purcell, Q.C., the advice—"That the Local Government Board have no power to delegate to Boards of Guardians any authority to dismiss or suspend the medical officer of the Union, and that the general orders to this effect in the Articles 39 and 40 of their recent circular are *ultra vires* and illegal."

The Irish Local Government Board has been referred to the sections of the Act of Parliament, as above quoted, and it seeks refuge in the third section of the same Act, which says that—

"The Commissioners are authorised . . . to make and issue orders for the government of workhouses . . . and for the guidance and control, appointment and removal of the officers thereof, and for guidance and control, *according to the intentions of this Act*, of all guardians, wardens, and other officers."

The Board appears to hold the opinion that this section gives them power to authorise Boards of Guardians to dismiss officers, in which view not only I, but much higher legal authority, entirely disagree. The Board may, under this clause, make any rules it pleases, "according to the intentions of the Act;" but it has no power to make any decree which goes in the smallest tittle against or beyond the words of the law.

The only legal precedents bearing upon the question are the cases of Dr. Kenny, of the North Dublin Union, and of one M'Gwiggan, an assistant schoolmaster. Dr. Kenny, it may be recollected, was dismissed by the Local Government Board for a political offence, and he sought to compel them to reinstate him, on the ground that that Board had no authority to "deem" him "unfit" unless for some cause conversant with the discharge of his medical duties. The case was argued in the Rolls Court, but before the judgment was delivered the Local Government Board submitted and reinstated him in office. In this case, therefore, no question arose as to the power of the Guardians to dismiss, and the only question which did arise—*i.e.*, the limits within which the Local Government Board had dismissalal power, was not decided.

More recently, the right of the Guardians, under the new rules, to dismiss was raised in the case of an assistant schoolmaster at Belfast, named M'Gwiggan. The cause of dismissal was not in dispute, but the right of the Guardians; and, in order to prevent the establishment of an adverse legal precedent, which might afterwards operate against the medical officers, the Irish Medical Association thought it necessary to intervene.

The case was argued in December, 1885, in the Queen's Bench, on behalf of the Association by Mr. John Gibson, Q.C., afterwards Solicitor-General for Ireland, and Mr. Monroe, Q.C., who was then Solicitor-General, and is now the Judge of the Land Court.

The judgment delivered by Judge O'Brien, on behalf of Chief Justice May, and Mr. Justice Johnston, would be incomprehensible to most of our readers, but I quote the important phrases:—

"As to certain principal officers, such as masters and doctors, the Local Government Board have an absolute power of dismissal on any grounds that they consider sufficient, so that, as to them, there can be no contract implied." . . . The common sense of the matter would seem to be to regard Boards of Guardians as a kind of artificial body for carrying out public functions governed and controlled in all their functions by the rules made concerning them.

"They are to appoint officers, but subject to the approval of the Commissioners. The latter are to make rules for the guidance, control, and removal of officers. Under section 31 they may direct the mode of appointment and determine the continuance in office and removal of such officers. Under section 33 they may themselves remove any officer whom they deem unfit or incompetent.

"The Guardians may suspend certain officers, but they are to report the cause of the suspension. They have absolutely no power of dismissal except as to the porter and assistants (to whom are added, in the English rule, nurse and servants—additions which indicate still more decidedly the precarious nature of the employment)."

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ART. XIII.—*Goitre in the Himalayas.* By BRIGADE-SURGEON  
WILLIAM CURRAN, A.M.S., Retired.

THE existence of goitre on the higher table-lands of Central Asia was first made known to us by Marco Polo. Describing the "Province of Yarcán," he says that "a large proportion (of its inhabitants) have swollen legs and great crops at the throat, which arises from some quality in their drinking water." Mir Izzat Ullah and Mr. Shaw confirm this; and he and other members of the expeditions that accompanied or followed him were overwhelmed with applications for remedies containing iodine by sufferers from this disorder. Mr. Schuyler adds, that goitre also

prevails in Khokand and Kashgar;\* and we learn from Dr. Hooker that it is very common "amongst the Tibetans, Bhotanese, and other inhabitants of the Nepal Terai." A diffusion so wide suggests a glance at its surroundings, and these may help to throw some light on its genesis and history.

Of its existence, and that, too, on a very large scale, within our own Himalayan territories there can be no doubt. The most careless or casual visitor to our hill stations must have seen scores of such cases; but it is certainly more common in some parts than in others, though why this should be so is not always apparent. It occurs at all ages and in almost equal proportions between Hindus and Mahometans, men and women, thus showing that its cause, whatever that may be, is no respecter of persons. It is rarely prominent, however, in either sex before the age of puberty, and its growth thereafter is slow and painless. It is nowhere regarded, so far as we could ascertain, as either a disqualification for matrimony or even a deformity. Anyhow, the subjects of it whom we examined appeared to make light of it, and they rarely, we believe, seek relief from it at the hands of either their own or European doctors.

Of its prevalence nearer home we are equally certain, though one would look in vain through Yorkshire, Derbyshire, or the valleys of Wales for anything like the "crops" here referred to; and it is frequently associated with cretinism in Italy and the Tyrol. The city of Aosta may, indeed, be regarded as its head-quarters; and it is in the valley that bears its name that, to use the words of Mr. Whymper, "these distorted mindless beings, more like brutes than men," called cretins, "most do congregate." Nothing of this, however, occurs in the Himalayas; on the contrary, their denizens are a hardy, well-developed race; and as to cretinism, pure and simple, it is conspicuous by its absence. Idiocy, however, is rather common in the interior of these hills; and one meets occasionally, in the remoter valleys, with a class of creatures who, for want of a better or more scientific designation, might be called

\* As this gentleman's testimony is very pertinent to our issue, I reproduce it here in full and will then leave it to speak for itself. Describing the city of Khokand, he says (*Turkestan*, Vol. II., page 15) that, "I was greatly struck with the prevalence of goitre, and it seemed to me as if every third merchant was afflicted with this disagreeable malady. Whether it has anything to do with the use of snow-water I cannot say, but the streams flowing through the city are formed by the melting of distant glaciers, and this was the only town in which I noticed this disease. I am told, however, that it is very prevalent in Kashgar and Yarkand."

"born fools." But the Himalayas are by no means peculiar in this respect, for there are, unhappily, examples in large numbers of that description of persons elsewhere, and it would be well for society and themselves that they were equally harmless.

Why this peculiar swelling or hypertrophy should manifest itself exclusively in this particular organ is not known; neither, indeed, is the function of this so-called *thyroid body* clearly understood.\* All we can say about it is that it is a soft, reddish, and highly-vascular structure, which forms a rounded projection on either side of the windpipe and larynx, and appears to protect these and also the great vessels of the neck as they emerge from the chest. But other swellings and diseases, such as cancer, tubercle, &c., are equally exclusive in their seizures; and, as in the one instance so in the other, glandular enlargements or degenerations are everywhere the heritage of poverty and want. Nor is this heritage confined to man—it is shared in also by his "fellow-mortals," the dog and other domestic animals; and it is often amusing to watch these creatures sluggishly moving about with "great crops at their throats," such as the famous Venetian traveller noticed elsewhere long ago.

Various causes have been assigned for its causation, such as locality, heredity, a poor monotonous vegetable diet, close stuffy valleys, and the habitual use of snow-water, or of water largely impregnated with lime. This latter is the cause to which the people themselves chiefly ascribe it, so that we need not discuss the others in any detail here. And as to the use of snow-water, goitre prevails in countries, such as Sumatra and South America, in which this water is scarcely ever seen, while it is almost unknown in Greenland and Lapland, where no other is at times available. Though willing enough to exhibit their crops to the curious traveller, the inhabitants of these hills rarely apply for treatment at his hands; and the only remedy they resort to is the old familiar moxa or tinder, which they burn over the tumour, and through which they sometimes succeed in diminishing its bulk, though they do not thereby entirely remove the deformity or subdue the disease.

As this moxa is regarded everywhere in the East, but especially

\* The recent experiments of Mr. Victor Horsley, of the Brown Institute, would seem to imply or establish an importance in the economy for this gland which was hitherto denied it. But the details are too technical for reproduction here, and the writer is only concerned with the popular side of the question.

so in the regions here referred to, as a panacea for all the ills that flesh is heir to, including amongst them this deformity, a few words on its mode of application and uses will not, we believe, be regarded as irrelevant or out of place here. It has been employed from time immemorial in these hills for pains of every description, and the people resort to it with a faith which no failure can chill and no discouragement or remonstrance can restrain. Infants but a few days old, if they appear to be in any way uneasy, are at once severely cauterised over their stomachs, and no bad effects are found to follow from this offhanded procedure. Veterans on the verge of the grave are similarly dealt with, and both appear to thrive under the infliction. The general way of applying it is to rub the part with a handful of cold wood-ashes and then strike it repeatedly with a piece of red-hot iron like Fig. 1. This produces dots or depressions like those indicated in Fig. 2, but the marks that are left by these repeated blows are, at least, twice as large as those given; and when this appliance is not at hand, another and even more painful method is resorted to. This is nothing more or less than putting a piece of lighted tinder on the part and allowing this to consume itself away. Though these remedies are certainly severe, no attempt is made to modify or evade them, and the eagerness with which they are resorted to on all domestic occasions would seem to imply that they are found effectual.

The Puhari doctors are of course wise enough to know that the rough method used some days or hours previously on the father in the fields would be quite unsuited for his infant offspring in the cradle. They discard accordingly the primitive-looking implement referred to above, and substitute instead of it an eyed, seton-like piece of metal, such as is sketched at Fig. 3. With this they produce some such blots or blotches as are represented in Fig 4, and it is no uncommon thing to see infants or children streaked in this way in this region literally "from the soles of their feet to the crowns of their heads." Nor is this procedure peculiar to this quarter. It is known or practised, in one form or another, over all the hilly ranges of the world. Its application is as common in Central Africa as it is in Central America, in Arabia as it is in Orissa, among the Philippine and Pacific islanders as among the wild tribes of the Rocky Mountains or the wilder inhabitants of the coast of Labrador. It has been used with benefit, so the patients acknowledged, in such widely different diseases as cholera and snake-bite, inflammatory fever and guinea-worm, and we ought

Fig. 1.



Fig. 2.

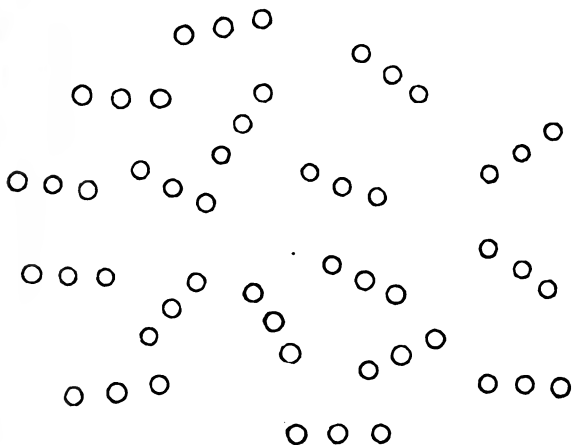
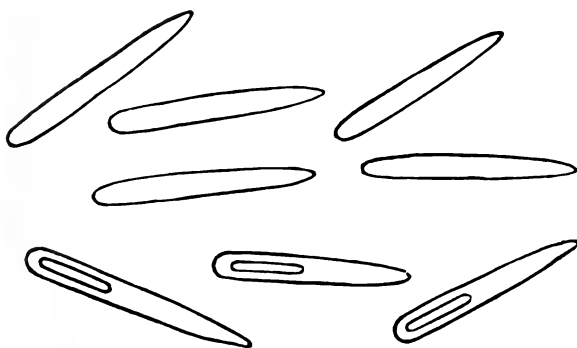


Fig. 3.



Fig. 4.



not therefore be surprised at hearing that it has frequently dispersed a voluminous goître. But the therapy of this swelling was not included in our original conception of the scope of this essay, and so we need not encumber these pages with any further reference to this feature of our case.

The fact mentioned by Dr. Livingstone, that drinking the water of Tanganyika proved a perfect cure in a few days to many of the goïtrous slaves whom he met in that neighbourhood, seems to imply that peculiarities of the soil or water supply of the district whence they came had something to say to it. The natives of the Himalayas are so convinced of this that they often change their abodes, but they cannot so easily change the hereditary predisposition their progenitors impressed upon them, and their food remains, unfortunately, always the same. This consists, in ninety-nine cases out of every hundred, of one or two small varieties of grain, which they call *Moorheea*, and which is simply a mixture of *Penicillaria spicata* and *Holcus sorghum*—in other words, of the bulrush millet and great millet. This is made up into a mess with some green vegetable, or seasoned with red pepper, and fried on a pan like the Scotch cake; and if we throw in a little milk and clarified butter, when these can be obtained, we have before us, in all its monotony, the cuisine of the Puhari, or hillman of the Himalayas.

Whether the vicious habit of breeding in-and-in, which is so common in these hills, has any influence on its production, and, if so, how much, we cannot say. These primitive people do not understand such questions, or, if they do, they are unwilling to speak about them; and it should never be forgotten in this connection, that goïtres are not regarded as a blemish in these hills, or a disqualification for any of the courtesies or amenities of social life. "In France, Italy, and Switzerland, it is a positive advantage," says Mr. Whymper, "to be goïtred, as it secures exemption from military service; and it is," he adds, "an unquestionable fact that the perpetuation of the great goïtrous family is assisted by this very circumstance." This is putting a price on deformity with a vengeance. But such is really the case—a goître is worth so much in hard cash in those regions in which the conscription obtains; and, describing a similar state of things in the Tyrol, the authoress of "German Home Life" adds, that "the fact of the goïtrous marrying the goïtrous, gazing upon the goïtrous, and living in a goïtrous atmosphere from time immemorial, has more

to do with the production of this hideous throat disease than the presence of dolomite rocks, snow-water, or the other remote causes assigned for it by science."

The unavoidable suppression—on account of the costliness of their reproduction as illustrations—of the five photographs with which this paper was accompanied, and some of which were obtained with difficulty in the interior, imposes on me the necessity of saying something about them as well as about the subject with which this essay is chiefly concerned.

Two of the photographs were taken in Kumaon, near Nynce Tal, and the remainder were secured at Mussoorie or in the adjacent hills. They represent two brothers, who were either coolies, cultivators, or shikaris as opportunity offered or occasion required; and two of the others represent a mother and daughter, who were charcoal-burners in the Teree Rajah's territory, near this latter station. The fifth was an imbecile old Hindu who lived on his son's bounty—and all are typical illustrations of the condition they are intended to portray. The former were healthy, vigorous, young men, of the middle height, who carried my cot, tent, &c., without a murmur over passes which I could scarcely get through with difficulty, and both laughed heartily at an avowed apprehension of mine that this deformity might interfere with their prospects in the matrimonial market. They assured me that they could get as many wives as they could maintain, and this excrescence did not "bar," they said, their or their neighbours' prospects in this or any other walk. A very similar feeling pervaded the other sections of this community I came across, and the Horatian maxim "*Nil admirari*" would form an appropriate motto for any or such symbol, device, or standard as the future ruler of their territory may set up.

Touching the old Hindu and his hebetude, I noticed several others in a similar condition, and there can, I think, be no doubt but that, though this condition produces no appreciable ill effects on the health of its owners in early life, it does so unmistakably as old age approaches, and I saw one or more illustrations of this at Gangoutree, near the source of the sacred Ganges. The old women who have been born with enlarged thyroids would pass without any preparation for Macbeth's witches; more hideous hags are not to be found on the surface of this gnarled globe. They all sternly refused my polite request for a "sitting," but I could easily see that they were not comfortable, and I should opine that these hard and stony masses would ultimately interfere with, if



they did not actually arrest, the functions of the trachea and the œsophagus, but I had no proof of this. Many, indeed, had already attained to extreme age, and I saw only one case in which this petrified mass had so far encroached on the chest as to *appear* to be eating away the clavicle and sternum. That it must also interfere in time with the nutrition of the brain by obstructing the flow of blood to it from the heart can scarcely be doubted, and this would be my explanation of the mental incapacity or decay that is so often noticed in connection with this condition in advanced life.

As regards the ætiology of the complaint I have not much to say. Most of those I conversed with assured me that they were born with it. "Cub se shuru hua?"—"When did it begin?"—I used to say to them, and their almost invariable answer to this interrogatory was "Paida hua, sahib," or shorter, "Paidaish se, sahib"—"From my birth, sir;" and there is no doubt in my mind but that heredity plays a large part in its propagation and genesis. So do, I am convinced, breeding in-and-in, and the defects (nutritional or otherwise) engendered by these are enhanced by their poor surroundings and the poorer monotonous vegetable dietary already referred to. Millet constitutes, as I said before, the basis of their food, and though this may be of kin with that *Revalenta Arabica* of which we hear so much, it only consists, according to the late Dr. Edward Smith ("Foods," p. 162) in a 100 parts—of water, 13; nitrogenous, 13; carbonaceous, 74; fat, 2.6; and salts, 2.3. Johnstone, or rather his editor, Church, describes ("The Chemistry of Common Life," p. 222) a beer that is made out of this seed by fermentation by the Kaffirs as "a sort of spirituous gruel of a very fattening quality" but these poor people are not fat—quite the contrary—and they know nothing of "fermentation."

Their beverage is "pure element" from the neighbouring brook or rill, and though this often comes from a distant glacier, or is even largely impregnated with lime, I do not think that it has much to do with the diffusion or genesis of this disorder. I say this, however, guardedly, and in face of the fact that many of the sufferers from it are otherwise minded, so indeed are many medical men, and some of the facts already disclosed within would seem to point in the same direction. But other facts are equally cogent in the other way, and there can be no doubt as to the purity of the source or the quality of the supply with which these poor people

are so abundantly, and withal so gratuitously, provided.\* We must therefore, I think, look elsewhere than to this substance for a key to the interpretation of the phenomena that precede or accompany this disease, and this key will, I think, be best found in an exhaustive analysis or judicious combination of the other cases enumerated above.

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ART. XIV.—*Experimental Researches upon Tuberculosis and Scrofula.* By EDWIN WOOTON.

[Continued from page 47.]

THE assertions made by certain eminent scientists that tuberculosis and pneumonia are due to certain lower definite forms of life—that is, are invariably of a specific septic character, has, I am sorry to say, met with a very general acquiescence without sufficient research or consideration.

That septic material taken from a tubercular subject and cultivated, may, by its injection into another animal's body, produce tuberculosis, proves nothing beyond the fact I have demonstrated, and apparently not grasped by the advocates of the specific septic character of tubercle, that any septic matters finding their way into the lymphatics will cause lymphatic inflammation, and, therefore, set up tuberculosis to a greater or lesser extent.

The asserted specific microbe of pneumonia has no more connection with this disease than any other form of life. Pneumonia can be produced, as shown already, by any of the septic forms, or by any other means of irritation.

A logical fallacy runs throughout the whole of the arguments of Koch and similar thinkers on this subject of septicism. It is assumed that because a particular thing can produce a disease, the disease is invariably caused by that thing; whereas, the conclusion can be relied on as accurate only when every attempt to produce the disease by other means has failed; and, since I have by experimental work proved that tuberculosis and pneumonia may be brought about by any irritant to the lymphatic system, the pseudo-microbes

\* The weight-on-head-carrying theory I purposely exclude from this list, for that best of all reasons that I do not believe in it, for though this mode of conveyance is very common in these hills, infants at the least cannot be turned to account in this direction; and I fancy that were we all to take to walking on our heads instead of as now on our feet, we should not even then become affected in this way without the aid of the other accessories mentioned within.

of tuberculosis and pneumonia resume their true position as merely accidental forms in a common state of septicism.

### III.—THE THERAPEUTICS OF TUBERCULOSIS AND SCROFULA.

Of the so-called "systems of treatment" and "infallible specifics" for pulmonary phthisis that have, from time to time, occupied the attention of the profession, I have tested a large number in the laboratory and have found that those having any beneficial influence on the disease consist, in every case, of one or more means acting on one or more of the pathological processes involved, and never on the whole of such processes. Hence, while all such systems and remedies have proved efficacious in certain cases, in others they have totally failed to cure; this is due to the facts that the pathological processes of tuberculosis vary greatly in intensity in different subjects, that many of them obtain only at certain stages of the disease, and that when they obtain throughout, or in more than one stage, they do not exert an uniform influence during the whole period of their subsistence. These considerations are applicable not merely to brutes but to the human subject. To instance their truth:—Iodine has been lauded as a specific and failed; its action is that of an absorbent; at the commencement of tubercular disease, therefore, it is a rational medicine, but the later processes are but slightly affected by it. Tar water, another "specific," is a valuable antiseptic, and consequently of service when septicism obtains, but altogether useless under other conditions. Tartar emetic lessens the blood-supply to the lungs by depressing the circulation, but this vaunted curative agent is of service only when a pulmonary lesion has involved blood-vessels of size, giving rise to hæmoptysis. Salts, both phosphates and hypophosphites, cod-liver oil, and other nutritive medicines, while affording material for lung tissue, cicatricial growth, and nervous energy, are totally useless to suppress septicism, and any treatment of this kind becomes a battle between normal cellular growth and septic irritation, the tendency of the latter being towards suppuration.

The "antiseptic system" is really curative of one only of the pathological processes, and if the subject be not in the stage when this obtains, the phthisis will proceed unchecked. It is to be remembered that, although septicism nearly always occurs before death, it is not essential to a fatal result.

Electricity again, most valuable as a physiological stimulant, provides no food, and is not antiseptic.

These examples will show how partial and incomplete has been the treatment of tubercular consumption, for every agent mentioned has in turn served as a "specific."

Medical skill is nothing but the logical application of facts, and the difference between the oft-styled cleverness of one practitioner and the stupidity of another—for the laity express their minds pretty freely concerning the profession—is the exact equivalent of the difference in the logical accuracy with which the knowledge of the respective "doctors" has been applied.

Placing on one side those engaged in research and the practice of special departments of the profession, there is not nearly sufficient difference in the extent of the knowledge possessed by practitioners to account for the marked contrasts in the success of their several treatments. Not infrequently the balance of knowledge between some two is so placed as to render the case apparently anachronistic—that is, the less learned practitioner is the more successful. I have known a medical student, whose acquaintance with *materia medica*, therapeutics, and medicine would certainly have failed to pass him at any examining board, treat severe and complicated diseases with far better results than the very men who were his teachers at the hospital. Moreover, his prescriptions were original. The whole explanation is—his mind was logical, and he applied logic to the facts set forth in his text-books.

In the cases of tuberculosis and scrofula we have, on the one hand, a series of connected pathological processes; and, on the other, a series of agencies by which these processes are capable of being controlled.

If, then, we understand the right relationship to each other of these pathological facts, and can determine when they obtain in the body, we should be able by the use of these agencies to bring about the physiological states constituting health.

This is not a mere logical theory, but a fact demonstrable both in the laboratory and at the bedside.

For the moment ignoring the facts these researches have revealed, and taking orthodox pathology and therapeutics as they were in 1864, 1874, or 1884, we are brought face to face with a sufficiency of information theoretically to yield results far more successful than those social and medical history would teach us have been obtained.

Whence, then, the failure?

Examine any leading orthodox work on medicine of the dates given and you will find therein valuable prescriptions and directions

for treatment, but without accurate and definite instructions as to the particular pathological stages in which they will prove efficacious. Moreover, when the pathology is dealt with therapeutically, the advice given does not cover the whole of the bodily processes involved.

Very frequently the instructions grossly violate pathological indications. Thus it has been taught: "If the patient does not progress under the influence of iodine, try inhalations of tar." "Iron has, in some cases, proved beneficial." "As to climate, some do well in a dry, others in a moist atmosphere." "A case is reported in the *Lancet*, of such a date, in which great benefit was obtained from \* \* \*." "Dr. So-and-so has had successful results with this or that," &c., &c., *ad nauseam*.

Here the treatment has been altogether worse than empirical, for the knowledge of facts possessed has not been applied in a logical manner to the conditions to be remedied.

It is sufficiently distressing to find medical scientists attempting to remove one of many connected processes obtaining at the same time, and very often choosing the more proximate on which to operate, as if a successful result in the case of any one such process would of necessity produce a cure of the disease. But it is worse by far when we find that absorbents, antiseptics, tonics, and other descriptions of therapeutic means are tried one by one, as if their applicability to the condition of the patient could be tested only by experiment.

There is, perhaps, one thing yet more sickening, to which I will only allude—the flaccid helplessness of the teacher who looks from the patient to the cemetery gates and moans—We have no possible means of cure, and can, therefore, only endeavour to postpone the fatal issue by doing our best to support the strength and treating symptoms.

From the records of cases that have come into my hands I have found large numbers of patients to have undergone a treatment consisting of particulars altogether misplaced. Tar water, when there was no septicism; tartar emetic, when the blood pressure was low; iodine and sea air in the far advanced stage, when no amount of absorption could possibly effect a cure.

Every one of these agencies used in its proper place would have been of service, but, misplaced, the results have always been worse than *nil*.

It should ever be borne in mind that there is no chance work

either in pathology or the therapeutical action of drugs of certain composition or nature. Differences in susceptibility exist—that is, dosology is not an exact science, but the action of every drug is constant for every human being, and, so far as research has extended, for every inferior mammal. Thus, iodine is always an absorbent, and, although insusceptibility to its influence may be very marked, its action can be obtained by enlarged doses.

I might illustrate the historical treatment of phthisis by likening the disease to a hostile army occupying a conquered country, and the resources of medical science to the army of that country. What would be said of any general of the defensive forces who, instead of bringing all available means to bear on the foe where they would be most effective with the object of rendering the artillery power of the other useless, the cavalry powerless, and the infantry incapable of maintaining their position—in short, by military skill and prowess overmastering his enemy—should try first his artillery, then, when it had failed to achieve aught, his cavalry, and next his infantry, and should use them, moreover, without regard for their fitness for the task assigned them!

Ridiculous as a cavalry charge against the walls of a fort may appear, it is not more absurd than the application of therapeutic agencies in the history of even recent medical science.

While the therapeutic battle has raged fiercely on the field of pulmonary phthisis, tabes mesenterica and tubercular meningitis have been comparatively free from warfare. In plain language, medical scientists appear to have agreed that aught approaching a cure in either disease was not to be looked for.

It may be admitted that while the pathology of tuberculosis was unknown, it was not in the power of medical men to effect the absolute removal of either condition; but, certainly, the logical application of known therapeutic agents would have relieved the diseases named of their severity, and prolonged life.

I am justified in coming to this conclusion by the results yielded by laboratory experiments on living animals, as well as by the effects produced in the human subject by therapeutic means used merely in accordance with the pathological knowledge of the past.

It having been seen that the pathological conditions in both diseases are incompatible with the due fulfilment of one or more of the body's functions, the first indication is the removal of these states for others approaching as nearly as may be to the normal.

There are certain first or general therapeutical principles applicable to the two diseases, and deducible directly from a knowledge of their pathology. The practicability of any measures based thereon is determinable only by the therapeutic means at command, and dependent, therefore, on experimental tests concerning the latter's value.

These principles are:—

1. Removal of constitutional condition by general measures.
2. Removal of local affections by general measures.
3. Removal of local affections by local measures.

It is evident that absorption secured in tuberculosis, and nutrition in scrofula, would, theoretically, result in the removal of the ensuing processes. But not only does direct action on the factor processes aid the result aimed at, but action at the primary source alone frequently fails, owing to fatal results being brought about by the diseases before the measures adopted have had time to accomplish their intended work.

All vital action, whether physiological or pathological, is essentially nervous in character—that is, conducted through the nervous system. Hence all such action can be affected by operating on the nervous centres. A perfectly healthy nervous system must result in perfect health wherever that system extends.

But here, again, laboratory experiments teach us that valuable and important as such central action is, we cannot, owing to its slowness of operation, afford to rely on it alone, and dispense with more local remedies.

But medical science has seldom erred on this side. Indeed, one of the gravest charges that can be brought against therapeutists is that they have persistently refused to see the dependence local health had on that of the nervous system. The treatment of all disease has been, generally speaking, in my opinion, far too local in character.

It will have been seen that scrofula and tuberculosis are two conditions involving a series of identical processes, the individual factors, however, not coinciding each for each—scrofula commencing with malnutrition, and tuberculosis with impaired absorption.

Bearing in mind the further facts with which we have become acquainted, the physiological processes we have to seek to bring about are, therefore—

In tuberculosis—Absorption in intestines and tissues, prevention of congestion, nutrition, prevention and cure of septicism.

In scrofula—Nutrition, digestion, renal action, absorption.

Every one of these physiological processes is obtainable by the action, in each case, of a particular class of agents.

We know the nature of the disease. We know the bodily and other processes which will remove it. And lastly, we know the agents which will bring about each of these processes.

Our task is, therefore, merely to apply these agents rightly, and to do this we must understand in general:—

1. How these processes best affect the diseased system—singly or in combination.

2. The physiological channel by which each process is best affected—singly and in combination.

3. The known agents affecting each physiological channel.

4. The comparative efficacy of their single and combined action.

And we must understand in each particular case—

5. The pathological conditions obtaining.

Individual remedies may be found of far greater power than many of those I have employed, and which, on account of variations from the latter in their character, may necessitate other methods of application; but if my pathology be true, the therapeutical principles I have laid down are of necessity true also, and it is only in accordance with those principles that agencies of whatever nature can be efficaciously employed.

The space at my disposal is so limited that I purpose omitting the minute details recorded in my MS. notes, and giving only a summary of the results obtained and their consequent indications. I am the more free to do this, as the accuracy of the statements can be tested by any medical scientist.

The experiments in connection with the therapeutics of tuberculosis and scrofula were undertaken with the object of finding the effect on the diseases in their various stages of each of various classes of agents, and of the latter in various combinations. Theoretical considerations were entirely put on one side, and no conclusions formed until the termination of the various tests. The results obtained were, however, entirely in harmony with the indications to be derived from a logical consideration of known facts, pathological, physiological, and therapeutical.

It was never within my view to test the action of untried drugs. Rather was it my object to master the correct application of those known to us. It is but of scanty service to humanity to add to our list of resources when we have means at command whose power has been proven, but is misapplied through lack of method.



The following table exhibits in, I trust, a lucid manner the scientific treatment of tuberculosis:—

Form of Disease	Physiological Process indicated	Channels by which Process may be brought about	Class of Agents by which effected
TUBERCULOSIS in all its forms	Absorption in tissues and intestines	Internal stimulation of lymphatics Local stimulation of lymphatics  Internal stimulation of nervous centres Local stimulation of tissues Nutrition of nervous centres, organs, and parts	Internal absorbents  External local absorbents Electric, thermal, frictional, and medicinal stimuli, exter. Internal stimulants and tonics  Electric, thermal, frictional, and medicinal stimuli, exter. Foods, medicinal and dietetic
	Nutrition	The processes bringing about absorption result in this	Do.
	Repression of septicism	Absorption and nutrition Free antiseptics in blood Antiseptic inhalation	As above Medicinal antiseptics internally Atomised and vaporised antiseptics
	Prevention of congestion	Free surface circulation Nutrition	As for nutrition—external stimuli —
	Relief of con- gestion	Relaxation of surface vessels Cardiac action Contraction of vessel walls	Rubefacient applications—med., thermal, and frictional Stimuli, general and cardiac Stimuli and tonics
	Arrest of hæ- morrhage	Contraction of vessel mouths and coagulation of blood Lessened cardiac action	Astringents internally and by inhalation Depressants
PULMONARY TUBERCULOSIS only	Lessened ex- pectoration when excessive	Nutrition Contraction of vessels	Which see Astringents by inhalation
	Freeing mucus from bronchi	Increase in contractile power of bronchi	As for nutrition—stimulating medicinal expectorants, electric and frictional stimuli
	Freeing mucus from bronchi when latter are clogged and there is danger of col- lapse	Enlargement of calibre of bronchi by relaxation to permit passage of air behind clot, followed by Increase in contractile power	Laxative expectorants  As above

Again, while it has been sought to compare with each other in each of various classes of drugs the more important of their respective members, it was never for one moment designed to form anything like a complete list of remedies. The great end in view has been the proof by experiment as to the classes of agents that will bring about the desired physiological processes, and the modes of application by which this can be best effected.

It may be argued that analogy cannot be made to point from a known process or fact in a brute's body to a concluded like condition in the human subject. As all physiological and pathological knowledge is in favour of a true analogy holding, I might leave the oft-repeated falsehood to its fate; but I desire to point out that these researches deal, save incidentally, only with the brute creation, and I therefore urge hospital pathologists and physicians to test by all practical means at their disposal the extent to which the observations I have recorded hold good in the human subject.

All the therapeutical experiments I have undertaken have, without exception, combined to lead me to the conclusion that the tuberculous condition is most rapidly removed when all the physiological processes negating those of a pathological nature which obtain are set in action simultaneously—that is, the processes act best in combination. It is therefore necessary to combine only agents which do not exert on one another a neutralising action.

#### THE VALUE OF CERTAIN KNOWN AGENTS.

Class 1. *Absorbents*.—There are in the British Pharmacopœia only two drugs the preparations of which promote directly the process of absorption to any appreciable extent—Mercury and Iodine.

The most powerful preparation of mercury for inunction is the red iodide, next to this the green iodide, and third on the list may be placed the perchloride.

In all forms of tuberculosis where marked local symptoms are present, local inunction is of service. In general tuberculosis without local symptoms the seat of inunction should be the abdomen.

For fumigation the most powerful forms of the remedy are the metal and its two iodides. The subchloride is comparatively weak in action.

When taken by the mouth, the most powerful preparations of mercury are those advised for inunction.

The facility with which the drugs act on the system through the channels named is in the order—mouth, inunction, fumigation.

When these channels are combined the absorbent effect of the mercury is more readily obtained than by any one such channel alone. Nor is this due solely or mainly to the increased quantity of the material employed. For example, if three grains of the green iodide be taken internally as a daily dose for fourteen days, the beneficial effects will be far less than those resulting from the employment of the same quantity divided into equal parts for the three channels named, and this notwithstanding the inevitable loss resulting from inunction and fumigation.

Mercury used in this triple manner affects the whole absorbent system more rapidly than when applied through only one such channel, and salivation under careful treatment less frequently precedes the desired physiological results.

Considered at its best, it is, however, an undesirable medicine; for, although in careful hands it may be made to do its duty without any evil after-effects, an error concerning constitutional susceptibility, or ignorance concerning a preceding mercurialisation, may be productive of much injury. I do not speak parenthetically; for, although my experiments have been conducted on brutes alone, it has been easy to ignore known peculiarities and previous treatment, and thus to bring the case to the level of those found in daily practice.

There is yet another channel by which mercury may be given—the lungs. Inhalation is, however, so rapidly productive of salivation and other dangerous conditions as to be altogether impracticable.

The second great absorbent—iodine—may, like mercury, be given by the mouth, inunction, fumigation, and inhalation. Internally, free iodine, its tincture, iodide of ammonium, and iodide of potassium, follow this order with regard to their readiness of action.

By inunction the same order is preserved—the crude iodine should be made into an ointment with lard without any other addition.

By fumigation—crude iodine, vapor iodi, iodide of ammonium, and iodide of potassium.

By inhalation—vapor iodi, and the iodides of potassium and ammonium; crude iodine is irritating when any pulmonary lesion exists.

In the case of any one of the above drugs its beneficial effects

are increased by its employment through more than one channel. But this increase is yet greater when for each channel is used the particular preparation most suited for it.

Iodine is a far more valuable absorbent than mercury, but constitutional peculiarities with regard to its action are of very frequent occurrence both in the laboratory and at the bedside. These peculiarities generally take the form of increased susceptibility to its influence; deficient susceptibility is very rare.

When the tubercular diathesis obtains, but without marked local symptoms, the inunction should be performed over the abdomen. This, with the internal administration of the drug, should suffice so far as itself is concerned. Inhalation, when no pulmonary symptoms occur, is not advisable, as the lungs are very readily irritated by the iodine, and local effects in such cases certainly precede constitutional.

It must be remembered that the action of iodine is always absorbent. When not concerned in the removal of effete or deposited material, its energies are expended in the absorption of the normal tissues.

It has been shown in a preceding article that it may be made to play an important part in the production of lymphatic exhaustion. This, however, is not so much the danger of its employment as an inhalant in general tuberculosis. That which results when no local tuberculous material is there for removal is the wasting of the lung tissue—pulmonary atrophy. The walls of the local vessels become weakened, and the local lymphatics enfeebled in their absorbent power.

When local tuberculosis obtains, the inunction should be effected over the region concerned. Fumigation may be practised under any form of the disorder, but inhalation is permissible in pulmonary phthisis alone.

**Class 2. Astringents.**—These are of service only in hæmorrhage and excessive expectoration. Internally there is but little choice between gallic acid, catechu, and krameria. These three are, in my laboratory experience, the best that can be employed. By inunction, gallic acid is to be preferred. Astringents cannot with success be applied by fumigation. By inhalation, however, they may be beneficially administered when pulmonary symptoms demand their employment, and then the spray will be found preferable to any other atmospheric means. No astringent with which I am acquainted has as powerful an action as iron. Either the perchloride or the sulphate may be used. The objection to its employment is

its irritant character. Animals during its inhalation breathe very frequently with great difficulty and cough violently. This is more especially the case with the perchloride. It is necessary, therefore, to remove the spray the moment it is found the iron acts as an irritant. All powerful astringents when administered by the spray act best in tepid water.

Another powerful therapeutic agent is atomised ice water. The temperature of this may be graduated by having three or four vessels in use, and the addition to their contents of varying quantities of tepid or cold water. Atomised water just above the freezing point has in my hands arrested very severe pulmonary hæmorrhage without fail. The external application of cold to a vital part with the object of checking deeply-seated local bleeding is permissible only when other means have failed. It is one of the most dangerous agents we have at command, for the first effect, by checking the superficial circulation, is to engorge the more deeply-seated vessels, and hence it frequently, in the laboratory, produces a fatal effect before any action on the bleeding vessels can result.

**Class 3. Medicinal Foods.**—These are amongst the most valuable in our store of resources. Although ordinary dietary contains everything that is required for the maintenance of health in debilitated states of the system, when absorption, digestion, and nutrition are, to a greater or lesser extent, impaired, common food is not in that form which will most readily become incorporated into the system, and it does not, like the articles of which I am about to speak, present another desideratum—the greatest potential nourishment in the smallest possible bulk. All these foods may be taken either by the mouth or rectum. The rectum, as the sole channel, is useful only when the stomach, on account of its irritability, rejects food entering it.

As an adjunctive channel, the rectum is of great service. A third channel is the skin, and this may be made to serve either by placing the subject in the bath or by inunction.

Medicinal foods are useful under every form and in every stage of tuberculosis, but more especially in the far advanced stage of pulmonary phthisis. Indeed when a pulmonary lesion has taken place they are second to none among essential therapeutic agents.

A food whose virtues are insufficiently appreciated is glycerine. It exists normally in the nervous system in combination with other compounds. It is the basic ingredient of fat. It is capable by modification within the body of forming and assisting to form other

essential "proximate principles." That glycerine serves as a food in all these ways when taken in sufficient quantity is known to all physiologists.

The advantages of its administration is the labour thus saved to the system by the presentation to the tissues of an already elaborated material. Although an alcohol, it has no hardening effect on the nervous tissues; neither does it in any way injure the liver, or lessen the oxidation of the blood.

The antiseptic properties of glycerine will be considered later on.

*Fats and Oils.*—The physiological uses of these in the animal economy are familiar to all medical scientists. In atonic states of the digestive system, however, fat as a dietetic article is in great part wasted. But scientific pharmacy has rendered this fact nugatory, for in emulsified and pancreatised fat we have a material requiring no digestion whatever. The highest praise is due to scientific pharmacists for the preparations with which their skill has furnished the profession.

Cod-liver oil has been for many years called the sheet anchor of the physician in dealing with pulmonary phthisis. The metaphor may be completed by remarking that the anchor, though strong, has ever had such clumsily constructed flukes and fell on such a rocky ground that it had no hold, but let the poor ship Constitution be dashed hither and thither until it became a wreck.

It may be granted that unmedicated oil has this advantage over unmedicated fat—it does not need emulsifying. But saponification is an essential preliminary to its absorption. Hence, in atonic digestive states it effects but a minimum of good.

In pancreatised cod-liver oil we have, so far as can be, a perfect medicinal food, possessing all the nutritive potentialities of the oil, and requiring no digestion before its absorption.

All other dietetic articles can be given in a wholly or partly digested state, as the case may demand. Perfect predigestion of ordinary foods is not advisable save when the digestive system is much enfeebled, or when the food is particularly trying to the digestive organs, as is the case with oils, fats, and milk. I have found that a healthy animal maintained on predigested foods alone had its digestive powers impaired, and the evidence *post-mortem* would point as the reason the action the secreted, but physiologically inactive, because superfluous, fluids had upon the mucous membranes.

The salts of potash, soda, and lime are amongst the most important

ingredients of the tissues, and their value as nutritive foods is deserving of the highest appreciation. Under whatever form these salts may be given, they undergo in the system decomposition, but this does not lessen their value.

The nutritive powers of these salts are influenced by the elements with which the base is combined. Of these phosphorus is deserving of special consideration.

It will be dealt with more fully further on. Here it may suffice to say that the hypophosphites are more stimulating than the phosphates. When preparations of free phosphorus are given with the object of promoting nervous action, the phosphates are to be preferred to the hypophosphites as medicinal foods, for the reason that the phosphorus in the latter is in a low state of oxidation, and any excess of the unoxidised material in the system is certain to be productive of injury, not benefit.

Iron, manganese, and ammonia are true foods, as they occur in the body, but in such small proportions to the mass of chemical constituents, especially in the first two cases, that any deficiency can be supplied in a few small medicinal doses. Anæmia, long supposed to be due to a deficiency of iron in the system, a pathological error, found its orthodox treatment in ferruginous medicines. Iron was poured into the body in the fervent hope that with energy and perseverance enough might be got together to supply the chemical deficiency.

Mineralised monuments of illogical superstition! An ocean to fill a thimble!

These drugs being assimilable only in very small quantities as foods, it is inadvisable to give them chemically compounded with other elements it may be desired to offer to the tissues. Thus the phosphate of iron passes in great part through the body, unaltered and unabsorbed.

Oxygen, however, a veritable food, can be given in the black oxide of manganese. The value of oxygen is partly as a food, partly as an antiseptic. When the lungs are healthy, a fully sufficient quantity of the gas is inhaled in any pure atmosphere for the body's well-being. When pulmonary lesions obtain, and the blood is imperfectly oxidised, oxygen inhalations will be found of service.

Internally, the manganese before mentioned and the permanganate of potash will readily yield the element. But the best compound with which I am familiar is the peroxide of hydrogen; it parts with its oxygen more readily than any other preparation.

The whole of it is volatile, if rightly used; it contains the gas in a larger proportion than any other compound—16 of oxygen to 18 of water. That with which it is combined is water, and therefore inert. In using it we are practically dealing with the element alone in a liquid form; it can be given—in practice—to patients who would shudder at the idea of an inhaling apparatus.

Oxygen as a food is, in tuberculosis, of value only in the pulmonary form, and then only when there is extensive lung lesion, or other pathological conditions preventing the normal reception of the element into the blood.

*Phosphorus*.—This element is a normal constituent of the tissues, but not in the uncombined or free state. It exists in the body under the forms of free phosphoric acid and phosphates. Free phosphorus does not occur in any article of diet, and while, when taken medicinally, it becomes a food by combination internally with other materials, it exerts special stimulant and tonic powers during the period of such elaboration. It is only in very minute quantities that phosphorus can be physiologically incorporated into the animal system. Doses of  $\frac{1}{30}$  and  $\frac{1}{30}$  grain given to cats and guinea-pigs, respectively, thrice daily for three weeks, I have found produce fatty degeneration of the liver and heart, and phosphorus was discharged, in various degrees of oxidation per anum. Now phosphoric acid ( $H_3PO_4$ ) contains 31 parts of phosphorus in 98 of the acid, and, therefore 30 gr. measures of the acid contain, approximately, 9 gr.  $+\frac{2}{3}$  of a gr. of phosphorus, and this quantity—that is, 30 gr. measures of the acid—I have given daily to cats, for periods of from one to five weeks, without producing any hurt whatever. In the form of phosphate and hypophosphite, as seen, this drug can be taken in large quantities with only beneficial results.

If we consider for one moment the amount by weight of the nervous tissue in an adult's body, and compare it with the minute doses of free phosphorus that can be given without poisonous symptoms occurring, we shall readily see, even dismissing from our minds the evidence I have brought forward, the utter absurdity of the hypothesis that the therapeutical effects] resulting from the medicine are due to its serving as *food* for a tissue in which its weight of phosphorus is contained many thousands of times over. As food, indeed, its action could be no greater on the nervous tissues than a few grains weight of meat daily on the whole body of a starving navvy.

As a therapeutic agent, taken daily for weeks or months, phos-



phorus requires to be administered with care. When the bowels are constipated, or rendered so by medicinal means, there is danger of poisoning, for the unabsorbed residue of the drug—and there is nearly always some amount—accumulates in the intestine, undergoing imperfect oxidation, and then entering the system not infrequently produces injurious and even fatal results. That which I have allowed to result in my experimental work, for the purpose of testing the influence of the drug, shows a possible danger, and may be a fatal fact in the history, unrecorded, of everyday practice—unsuspected, and, therefore, thrice deadly. In all cases of phosphorus poisoning, I have found the first seat of pathological change to be the liver. Phosphorus is a stimulant and tonic, and its proper application will be spoken of when dealing with this class of medicine.

*The Mineral Acids.*—Hydrochloric, sulphuric, and phosphoric acid have a two-fold action as food. First, that which they exert in the system when they have undergone absorption without change; and secondly, that which they effect in the intestines by their action on excreted and effete salts. These acids are not excreted in the free state but in combination, and chiefly by the urine.

Both in the blood and intestines they are conservators of energy by the chemical changes they effect in effete salts. Under their influence the body becomes better nourished, and the fæces poorer in their mineral constituents.

The proportionate quantity of the acid in each case absorbed unchanged, depends entirely on the presence or absence, and in the former case the nature, of material in the stomach and that portion of the intestines into which the acid may pass.

The presence of any combinable base or salt will cause a chemical reaction to occur, so that given before or after meals these acids, as such, do not undergo absorption.

Phosphoric acid in the free state occurs in the nervous centres, and the drug is therefore of value as a food. Also, by the conservative phosphates it assists to form, it is of service in furthering bone development in young children.

To sum up. While all the acids named are conservators of energy by renewing the activities of effete material, phosphoric acid is, in addition, a direct food for the nervous tissues. The pharmacopœial dose of this drug is altogether too small. I have frequently taken, when fatigued, three drachms in water, and have prescribed for brain-workers from one to two drachms, and never once with any injurious, but with, on the contrary, markedly beneficial, results.

But it must be understood that in none of these cases did any natural irritability of the bowels obtain. With a relaxed or irritable intestine, even a dose of thirty minims will produce diarrhœa. The common sense of a practitioner will dictate that he should satisfy himself as to the state of the bowels before prescribing the medicine, and to be certain of no injury arising through an error on this score he should give the drug in gradually increasing doses, commencing at, say, half a drachm.

With regard to brutes, the smallest dose I have (in experimenting on cats) found produce diarrhœa was twenty-six minims in three ounces of water. Some of the animals would take one drachm and a half without any subsequent morbid symptoms. When those who were affected by the minimal dose mentioned were subsequently made to swallow quantities varying from one to two drachms, the only effects, both when the acid was given strong and diluted, were vomiting and increased diarrhœa.

*Systemic poisoning by phosphoric acid is, in the laboratory at any rate, a physiological impossibility.*

*[To be concluded.]*

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#### HYPNONE : ANOTHER NEW HYPNOTIC.

DR. DUJARDIN-BEAUMETZ recently submitted to the Académie de Médecine of Paris the results of a series of observations on acetophenone, or, as it is more commonly called, hypnone. This compound is prepared by the action of chloride of benzoyl on zinc-methyl, or by distilling together a mixture of benzoate and acetate of calcium. It is a colourless mobile liquid, having an odour not unlike oil of bitter almonds or cherry-laurel water. It has a very decided physiological action, for a cubic centimètre injected under the skin of a guinea-pig produced a torpid comatose condition, from which the animal did not recover. The respiration was quickened, the heart-beats became fewer in number, the animal started convulsively, gradually grew colder, and died. To produce sleep, it should be given in doses of from two to sixteen minims; and, if administered at bedtime, it uniformly produces a well-marked hypnotic action. It may be diluted with alcohol, ether, or glycerine, but the best way to give it is in capsules. It communicates to the breath a somewhat disagreeable odour, but its taste may be masked by syrup of orange-flower or oil of sweet almonds. Dr. Dujardin-Beaumetz's observations have been fully confirmed both by Dr. Constantine Paul and by Dr. Huchard.—*Brit. Med. Journal*, Feb. 20, 1886.

## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

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*Transactions of the Academy of Medicine in Ireland.* Vol. III.  
Edited by WILLIAM THOMSON, M.A., F.R.C.S., General Secretary; Surgeon to the Richmond Hospital. Dublin: Fannin & Co. 1885. Pp. 468.

THE Report for 1883-84 accompanies the Transactions for 1884-85. We have no reason to doubt that the growth of the Academy during the latter session equalled or exceeded its progress in the second year of its sturdy infancy. In the quality of the papers submitted to the Sections there has been no falling off; but we observe that the number of communications is less, by 9 per cent., than that of the previous session, the diminution being most marked in the Medical and Surgical Sections. In each of these the number of papers read was only two-thirds of the corresponding number in 1883-84; and in the Obstetrical Section there is a numerical falling off amounting to 30 per cent. No address, either by the President or by a President of a Section, finds a place in this volume—an omission much to be regretted. Nor is any discussion reported, although it must often have happened that discussions “of importance and interest” followed papers communicated; and provision for reporting them is expressly made in rule 64. This same rule, by the way, gives the General Council power to decide what papers shall appear in the Transactions—a power, we suspect, rarely, if ever, exercised. Some flies are preserved in the amber of this volume, the omission of which would have made room for the more useful constituents indicated.

We turned with interest to Surgeon-Major Hamilton's paper *On Cholera*, which contains much valuable information on the subject, and adopts Dr. Bryden's views—that the disease is “earth-born and air-borne.” Written by an officer whose Indian experience appears to have been confined to Bengal and the North-West Provinces (as we infer from his remarks about “caste prejudices,” and his want of appreciation of what has been done for sanitary improvements in

other parts of India), it makes no attempt to adapt the course of epidemics in the south and west, or in Europe, to Bryden's theory. The task would, we suspect, have been a hopeless one. He examines, in order to condemn, "the theories usually set forth in opposition to" Bryden's, which he styles the "water theory" and the "human intercourse theory." As to the former, in spite of "Pettenkoffer" (*sic*) and Dr. Klein, we must continue to hold that the man who drinks water containing choleraic excreta runs enormously greater risk of an attack of cholera than if he drank of water equally foul in every respect *but* the absence of the specific poison—whatever this may be. It is not *certain* that he will be attacked, any more than that every unvaccinated person brought into close contact with a case of small-pox will take this disease, or every one bitten by a rabid dog will die of hydrophobia. But this is no argument against the "water theory." As to "human intercourse," we are glad to find it "admitted that, under certain circumstances, it may be a factor in spreading an epidemic." Even Dr. Bryden, we are told, allowed that cholera was probably contagious *in Europe*. If human intercourse, then, is, in any degree, instrumental in spreading cholera, why are we told that "quarantine has as much power to keep out epidemic cholera as the proverbial pitchfork has to keep back the tide?" Quarantine may be "impracticable," is undoubtedly "vexatious," but why "useless?" If the disease is "earth-born and air-borne," and there is no relation of cause and effect between one case of cholera and another, why should a vessel, "in the case of cholera having been on board, be disinfected, and the crew isolated for at least ten days subsequent to the appearance of the last case?" Does any rational being, except Surgeon-General Cuninghame and his handful of followers, doubt that if *no* person or thing were landed, directly or indirectly, from a cholera-infected place, on the coast of Ireland, we should be perfectly safe from "air-borne" infection? These denials of the existence of a poison of some kind produced by a cholera patient and transferable from him to other persons, and opposition to every precaution which can be called "quarantine," would be ludicrous if they were not so mischievous. Mr. Hamilton is, we are happy to say, healthily inconsistent; and the excellent advice he gives in this paper may be followed by any of us who may have to deal with this formidable disease in the coming summer. Happy they who, after experience, will be able to differ from the author's conclusion, "that the outlook, as regards the prevention and treatment of cholera, is dark indeed."

Dr. Finny's paper on *Hyperpyrexia in Rheumatic Fever* deserves special notice. The complication is a rare one—the Middlesex Hospital cases, 1869–80, giving a percentage of only 1·7. The mortality, in 67 cases tabulated in the Clinical Society's Report on Hyperpyrexia, was nearly 50 per cent. The usual explanation of the symptom—that it is due to the retention of some poisonous material in the blood following arrest of sweating—an explanation accepted by Dr. Bristowe, must be rejected. Dr. Finny remarks, "it was demonstrated that, of the 67 cases referred to, sweating was distinctly present in two-thirds, and sudamina were noted in 22; and that a dry, non-perspiring skin was present in not more than one-fourth of the cases. In both my cases sweating existed immediately preceding the occurrence of the hyperpyrexia, and sudamina were present in one." Other important questions are ably discussed in this paper; but our limits will permit notice of one point only. After examining the various methods of treatment recommended, he says:—

"The prompt and early application of cold to the surface is, therefore, the most valuable mode of treatment of the hyperpyrexia in acute rheumatism, as it is in the similar condition of insolation or heat apoplexy. The chances of its efficacy are greater the earlier it is had recourse to. The temperature cannot safely be allowed to rise above 105°."

Dr. Kennedy's *Case of Senile Dementia*, a not uncommon disease, of which restlessness is the most prominent symptom, illustrates the advantage of prescribing mercury, at least where chloral, hyoscyamus, and other more obvious remedies have failed to give relief. It illustrates also the fact (of which, however inexplicable, there is sufficient evidence), that many cases of insomnia or of imperfect sleep are benefited by turning the bed-head to the north.

Dr. Little's paper *On the Form of Pneumonia prevalent in Dublin* was intended to elicit and, we have no doubt, did elicit a discussion which added to its own value; but from the benefits of which outsiders are excluded.

Dr. Cruise's *Notes of Visits to Contrexéville and Royat-les-Bains* have already appeared in our pages. They are interesting, and will, no doubt, be useful to those of our brethren gifted with patients whose circumstances enable them to go abroad for health.

There seems to be some little uncertainty in the minds of "the authorities" as to the classification of papers. Dr. Wright's *Case of Sloughing of the Rectum* we should have expected to find in the

Surgical Section of the Transactions; and Mr. Thomson's *Three Cases of Ovariectomy* might have gone with Dr. Macan's fourteen in the Obstetrical Section. All Mr. Thomson's cases were successful. Of Dr. Macan's only one terminated fatally—giving a mortality of 7.1 per cent. The practical remarks following the abstracts of these cases are of great value.

Mr. J. K. Barton groups together six *Cases in Knee Surgery*. Two of these were successful excisions of the joint—one for disease, the other for deformity resulting from disease. A case of amputation through the joint was also successful.

Dr. T. More Madden's paper *On the Treatment of Uterine Fibro-Myomata* will be read by obstetricians with interest. He discusses hysterectomy, oöphorectomy, and the other intra-peritoneal operations; and, while fully admitting the enormous benefits due to the recent extension of abdominal surgery, he protests against "the spreading cacoethes operandi prevalent amongst abdominal sectionists of the Birmingham school." He quotes, in illustration, the language of one of these gentlemen, who—"in reporting upwards of a hundred cases of abdominal section undertaken within a year—boasts that he performs this operation in every case which he has 'the opportunity of operating on,' without 'the slightest attempt at the selection of cases, and at choosing the most suitable and rejecting those which did not seem to promise to be successful.' Nor does he hesitate to admit that, had there been such selection of cases, he would have had fewer deaths of patients on whom he 'operated with the idea of giving them the slight chance of life the operation afforded, knowing well beforehand how slight that chance was.'" Against this sort of thing we protest as earnestly as Dr. Madden, whose words we transcribe:—

"Believing, as I do, that abdominal operations unquestionably afford the only possible method of saving health, or relieving otherwise irremediable suffering in some cases of uterine fibro-myomata, I trust that the voice of the profession will make itself heard in no uncertain tone in reprobation of the operative *furore* which has manifested itself in some quarters with regard to the performance of abdominal sections for uterine tumours, and which else may lead to a reaction against the legitimate employment of procedures so valuable in suitable cases. In my student days I was taught that an operation was justified only as means of saving life or relieving suffering otherwise hopeless. It would now, however, appear that the gravest operations may be resorted to in every case in which the opportunity presents itself, and without any regard to their

probable consequences. For my own part, I am unable to accept this doctrine, and am still old-fashioned enough to believe that no operation—especially one of such gravity as that under consideration—should be undertaken save as a matter of necessity, and with a reasonable prospect of a successful result. If the indiscriminate performance of gastrotomy now advocated by some eminent specialists be unfortunately ever generally adopted, then the sooner the better it will be that those suffering from any of the diseases in which abdominal section is now abused should abandon surgical aid and trust themselves, as formerly, to unassisted Nature's kindlier euthanasia " (p. 246).

Another paper in the Obstetrical Section to which we think special attention due is Dr. Purefoy's on *Intestinal Obstruction* in children, particularly to the paragraphs relating to diagnosis (p. 266). We note that the clumsy old mechanical plan for the relief of intestinal obstruction by the dead-weight of mineral mercury has been successfully employed in three cases recently. The danger of puncturing the intestines to discharge flatus, also, is incidentally mentioned: "from some unexplained cause punctures made under these circumstances are apt to remain gaping, and thus, of necessity, a fatal peritonitis ensues, as happened in two cases."

The Pathological papers, with two exceptions, do not demand special notice. Mr. Charles B. Ball illustrates the connection between working with tar and epithelial cancer, by a second case—a previous one having been brought before the Section in the Session 1883-84. Of 19 men employed in Dublin in tar-distillation three have been operated on. It appears that workmen employed in the manufacture of carbolic acid suffer, in many instances, from warts; and, indeed, Mr. Ball's paper is followed by a case of Mr. Story's, in which epithelioma of the eyelid resulted from irritation by crude carbolic acid.

Mr. George Foy, in his remarks on *So-called Malignant Growths*, suggests a connection between the prevalence of cancer and "the scarceness or plenty of food stuffs;" points out the disappearance of "cancer scroti" from hospital reports; infers from the greater frequency of cancer in commercial centres, from "the influence of mental trouble in predisposing to it," from a case of Dr. Snow's, in which intense anxiety was followed by cancer, that controllable circumstances increase the number of cancer cases, and, consequently, that the influence of heredity has been exaggerated. He objects (as do we) to the term "malignant," a survival from days of darkness and ignorance.

A result, no doubt, of our "personal equation," an infirmity from which even reviewers are not exempt, we found the papers read before the Sub-Section of State Medicine the most interesting in the volume.

Dr. Cosgrave's short paper on *Sewer-gas and Disease in Dublin* contains nothing recondite; but it puts matters of the highest practical importance in plain language, and should be studied by all householders, doctors (who are not invariably as careful about sanitation in their own dwellings as might be expected) included. The instances of defective sewerage and its consequences, which Dr. Cosgrave adduces, are most instructive. We endorse his advice, that the examination of premises suspected to be defectively sewered or trapped should not be entrusted to an amateur or a plumber, but should be made by a competent sanitary engineer.

Sir Charles Cameron gives an account of the working of the Adulteration Acts in Dublin. The Act of 1860 seems to have been abortive everywhere else; but in Dublin more than a hundred convictions were obtained under it, most of them for adulteration of milk. Sir Charles assumes a standard for milk of 12 per cent. solids; but does not advise prosecution unless they fell below 11.5. His method of stating the proportion of water-adulteration is the more convenient, but not the more usual, and should be noted. He expresses the adulteration as so much per cent. added to the original milk. "For example, a gallon of water is added to a gallon of milk. It may, in such case, be said either that the mixture is milk containing 50 per cent. of added water, or that it is milk to which 100 per cent. of its weight [*sic*] of water had been added as an adulterant. I invariably employ the latter method." The "Sale of Food and Drugs Act" (38 & 39 Vict., c. 63, 1875) has been utilised to a large extent in Dublin, and with such success that "at present, very few articles are liable to adulteration save milk and butter. None of the specimens of coffee, tea, sugar, mustard, pepper, and liquors, lately collected for analysis, have proved to be adulterated."

Dr. Grimshaw discusses the relative strength of town and country populations in this country, defining "a town" to be a place with municipal government, having a population of 10,000 or upwards; and calling "the district in which that town was situated a 'civic' district, and the population of the whole district a town or civic population—the area adopted as comprising the civic population being the poor-law union or superintendent registrar's district in



which such populous place was situated." Thus classified, the mean civic population of Ireland in 1871-80 was 1,476,929, and the rural 3,816,678. The average annual mortality of the former was 22·5 per mille, of the latter 16·6, the difference being 5·9. It follows that the counties containing large towns show a higher death-rate; Dublin, Waterford, and Antrim heading the list. The "temporarily sick" of the census returns correspond: 7·75 per mille of population who were sick, being made up of 6·64 rural and 10·51 civic. We cannot quite share Dr. Grimshaw's apparent surprise at this superiority of rural populations. Townspeople should, he says, "be better housed, better supplied with water, . . . have better sanitary appliances, better cleansing arrangements," &c. We doubt if the poorest isolated cabin, perhaps "keeping open house for want of a door," certainly enjoying freest ventilation with the purest air, is not in better sanitary condition than the artisan's house in a city lane. The spring-well is a better source of water-supply than the house-cistern or water-butt, and the open field a wholesomer receptacle for excreta than the unsewered privy, or even the most scientific water-closet.

When we examine the causes of civic and rural mortality, respectively, we find that all the most important causes (except childbirth) are more active in towns. Taking zymotics, phthisis, and respiratory diseases (which produce more than one-third of the total Irish mortality), we learn the town mortality exceeds the average by 10·6, 8·1, and 11·4 per 10,000; and exceeds the rural by 14·5, 11·3, and 15·8. Taking phthisis and diseases of the respiratory organs together, as they obviously ought to be taken, we learn that, during the decade 1871-80, they caused more than one-fourth of the total mortality of the country, and that the mean civic death-rate due to these causes exceeded the mean rural by 27·1 per 10,000.

We quote, somewhat condensed, Dr. Grimshaw's remarks on puerperal causes of death. The tables giving the relative distribution of deaths, in civic and rural populations, from puerperal fever and childbirth show a result—

"Which differs remarkably from the results obtained where the town and country death-rates are compared for any of the other diseases or groups of diseases. . . . The death-rates to the population from puerperal fever and childbirth combined are almost the same in town and country—namely, 1·8 and 1·7 per 10,000 respectively; taken separately, however, the death-rate from puerperal fever, which is an infective disease,

is slightly higher in town than in country, while in other forms of death from childbirth the mortality is greater in country than in town. . . . The relation of deaths from puerperal fever and childbirth combined to births registered is, for the whole of Ireland, at the rate of . . . 6·6 per 1,000, for puerperal fever alone it is . . . 2·5 per 1,000, and for childbirth . . . 4·1 per 1,000 births. If, however, town and country are compared it appears that the mortality from puerperal fever in town districts is . . . 2·8 per 1,000 births, while in country districts it is . . . 2·4 per 1,000, being slightly less. On the other hand, the deaths from 'childbirth' are proportionately less in town than in country; in the civic districts the rate is . . . 3·6 per 1,000, while in country districts it is . . . 4·3 per 1,000 births. If the two be taken together the result is that deaths from parturition are at the rate of 6·4 per 1,000 births in town as compared with 6·7 in country districts. It is clear that this excessive mortality from parturition can be attributed only to the less favourable circumstances under which lying-in-women are treated in the country as compared with town. . . . The explanation must be in the comparative neglect with which lying-in-women are treated in the country parts of Ireland" (p. 401).

In the remaining paper of this Section, Dr. Jacob goes on the war-path valiantly but, we think, hopelessly against *Compulsory Notification of Infectious Disease*. Our profession is well represented in the present Parliament, and the case, as our champion puts it here, is a good one; but we cannot feel sanguine as to the result.

We shall notice but one other paper in this volume—communicated by Professor Cunningham from Professor Francis J. Shepherd of McGill University—*On the Musculus Sternalis in Anencephalous Monsters*. Dr. Shepherd's investigations confirm Dr. Cunningham's view, founded on the nerve-supply of this muscle, that the latter belongs to the pectoral group, and is not a remnant of the *panniculus carnosus*. It appears that the muscle is very common in this group of monsters, and that these are more frequently female than male.

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*Handbook of Diseases of the Skin.* Edited by H. v. ZIEMSEN, M.D. Illustrated with Eighty Wood Engravings and Colour Prints. New York: William Wood and Co. London: Sampson Low and Co. 1885. Pp. 658.

THIS Handbook of Skin Diseases is presented by the publishers to each of the original subscribers of "Von Ziemssen's Cyclopædia," and makes the Cyclopædia one of the most complete records of medicine

ever published. The publishers of this work have far exceeded the amount of matter they undertook to give their subscribers. The volume, strongly bound in cloth, is replete with information upon the special subject it deals with, contributed by several of the most eminent Continental authorities.

*RECENT WORKS ON MATERIA MEDICA AND  
THERAPEUTICS.*

1. *Von Ziemssen's Handbook of General Therapeutics.* In Seven Volumes. Vol. II.—*Antipyretic Methods of Treatment*, by PROFESSOR C. VON LIEBERMEISTER. *Antiphlogistic Methods of Treatment*, by PROFESSOR TH. JÜRGENSEN. *Epidermic, Endermic, and Hypodermic Administration of Medicines*, by PROFESSOR A. EULENBURG. Translated by MATTHEW HAY, M.D., Professor of Medical Jurisprudence in the University of Aberdeen. With 12 Illustrations. London: Smith, Elder, & Co. 1885. Pp. 511.
2. *The Essentials of Materia Medica and Therapeutics.* By ALBERT BARING GARROD, M.D., F.R.S., &c. Eleventh Edition. Revised and Edited by NESTOR TIRARD, M.D., Professor of Materia Medica at King's College. London: Longmans, Green & Co. 1885. Pp. 545.
3. *A Guide to Therapeutics.* By ROBERT FARQUHARSON, M.P., M.D. Edin., F.R.C.P. Lond., LL.D. Aber.; late Lecturer on Materia Medica at St. Mary's Hospital Medical School, &c. Fourth Edition. London: Smith, Elder & Co. 1886. 8vo. Pp. 394.
4. *The Prescriber's Pharmacopœia.* Revised and Edited by NESTOR TIRARD, M.D. Lond., M.R.C.P.; Professor of Materia Medica and Therapeutics at King's College; Assistant-Physician to King's College Hospital; and Physician to the Evelina Hospital for Sick Children. Sixth Edition. London: J. & A. Churchill. 1885. 12mo. Pp. 168.
5. *The Pocket Pharmacopœia for 1885.* By ARMAND SEMPLE, B.A., M.B., Cantab., &c. Students' Aids Series. London: Baillière, Tindall, & Cox. 1886. Pp. 192.

1. In our recent notice of the first volume of this Handbook, we called attention to the importance and character of the work as a

whole. This, the second volume, consists of a series of monographs dealing with some special methods of treatment and with some of the modes of administering medicines. The authors are men whose names are well known, and the translation has been excellently done by Dr. Matthew Hay, of Aberdeen, assisted, as he cordially acknowledges, by Dr. Baron.

The first article, by Professor von Liebermeister, is on "Antipyretic Methods of Treatment." This subject is discussed under four heads:—First, the methods of directly abstracting heat from the body, and the effects. Next, those medicines which have an antipyretic effect; then, antipyretic dietetics; and finally, the indications for the application of antipyretic treatment, and the results which the employment of it has up to the present yielded.

Notwithstanding what has been done by a few English and Irish physicians, there is still undoubtedly a prejudice amongst us against the methodical treatment of pyrexia in ordinarily severe cases of fever by the repeated immersion of the patient in cold water baths. And in this country, at least, we think it is rarely or never made use of, except, perhaps, as a *dernier ressort* in cases of urgent hyperpyrexia. The reasons for this "prejudice," if such it be, it is unnecessary to discuss here. If, however, statements supported by the weight of personal authority and scientific deductions, and by statistics deduced from thousands of cases, could convince anyone that the result of such a mode of treatment—in the absence of well-defined contradictions—was so satisfactory, and so superior to any other method, that it should be followed "in severe cases of fever as a matter of course," a study of this able and exhaustive article would probably do so.

Liebermeister has accurately investigated the extent of action, and the comparative effectiveness of the chief methods for abstracting heat. He is careful to point out that the choice of a method depends upon several circumstances, but that the employment of the ordinary cold bath is by far the most effective heat-abstractive method. After this comes the gradually cooled bath (von Ziemssen's method), then the wet pack, and finally cold affusion. The weaker heat abstractors—*e.g.*, cold spongings, compresses, ice-bladders, &c., are agreeable to the patients, and may, in certain circumstances, be a valuable auxiliary. But if their action on the temperature of the body is observed, and a calculation be made as to the amount of heat that can possibly be abstracted from it by such processes, even in the most favourable cases, it will be found that they are next to

no value as contrasted with the action of a cold bath. While they cannot supersede the latter, they are by no means to be rejected. "It was a step in advance which was made last century, when it was found out that we might, without hesitation, bathe the face and hands of a fevered patient with cold water; but it would be a serious mistake if, under the idea that we might effect a material result by means of the milder methods, we should in consequence neglect the more effective methods of heat-abstraction in any serious cases."

It is chiefly in severe cases of enteric fever that the cold bath has been employed; and numerous tables and observations as to the result of such treatment, and the influence the period of the day, &c., has in its employment, are given in the text. Cold baths appear to have, on the whole, a somewhat less effect in pneumonia than in enteric fever; and it is remarked that the fever of the former disease usually offers a greater resistance to other antipyretic agents as well.

Liebermeister acknowledges that the action of a cold bath is "very severe treatment for the majority of patients, especially if the baths have to be often repeated." This being so, we doubt whether it would always be so easy as the author thinks to convince the majority of our patients in this country, or their friends, "by reason," of its suitability or necessity.

The statistics adduced seem to establish that the existence of a catarrh of the respiratory passages is no bar to the use of the cold bath, and that affections of the pulmonary organs are less frequent in enteric patients thus treated, than they were prior to the introduction of this method of heat-abstraction.

Full and clear directions are given as to the procedure to be observed in giving the different kinds of baths described, their repetition when necessary, and the indications and contra-indications for their use.

The cold bath and other similar *antithermic* methods, by which heat is abstracted from the body, and its temperature thereby directly lessened, are not, theoretically, so perfect or so rational as those which act by diminishing the production of the heat, and which, strictly speaking, are the only true *antipyretic* measures. Of medicines possessing this action, Liebermeister's views as to quinine and salicylic acid need alone be mentioned, as he now rarely uses digitalis and has given up veratrine. When properly administered, quinine is a valuable and generally reliable antipyretic. It is elimi-

nated in a comparatively short time through the kidney; therefore a full antipyretic action can only be obtained when a large quantity of the drug is quickly absorbed. It is mere waste to give quinine, in the hope of its acting as an antipyretic, in the small doses we have seen practitioners prescribe in it. A dose of 20 to 45 grains is necessary for such a purpose in the case of adults; and it is also absolutely necessary, to secure a full antipyretic action, that the whole dose, if divided, be administered within the course of half an hour. Von Liebermeister accentuates this point, inasmuch as its non-observance was the reason why, at first, he and many other observers thought quinine was not an efficient antipyretic. As a rule, he does not repeat the dose until after 48 hours, and in no case until after 24 hours. It is of special interest to note that in healthy persons who have no fever, no perceptible lowering of the temperature of the body is induced by quinine.

There are various special circumstances indicated as influencing the extent of the temperature-reducing action of both quinine and salicylic acid—such as the largeness of the dose, the period of the day, the nature and intensity of the disease, its stage, and the idiosyncrasy of the patient. Quinine does not produce its effect until about 8 to 12 hours after its administration; and the time of its strongest action coincides, by preference, with the time of the spontaneous morning remission. Therefore, between the hours of 3 and 7 in the afternoon appears to be the best time for administering it. The antipyretic action of salicylic acid appears, on the average, considerably sooner than that of quinine. A definite diminution of the temperature may, in general, be observed within 1 to 2 hours after the ingestion of a sufficient dose, and the greatest diminution happens in about 4 to 6 hours after its administration. Hence, it ought to be administered late in the evening, somewhere between 8 and 10. In consequence of its comparatively rapid absorption, it is desirable not to administer the whole dose at a time, but to divide it into small doses, and give one of these every hour, or hour and a half. With adults, Liebermeister usually employs, for antipyretic purposes, a dose of an extemporised, freshly-made, saturated solution of salicylate of sodium, corresponding to 75 grains of salicylic acid. As regards its antipyretic action, he looks upon salicylic acid as a perfect substitute in every respect for quinine; but, in consequence of an impression that salicylic acid exercises rather an unfavourable action upon the heart, he inclines, in cases in which there are clear signs of cardiac weakness, to give the preference to quinine. But

if the action of the heart is strong, and its rate not too high, he regards salicylic acid as an antipyretic entirely free from danger. Another point is, that salicylic acid and quinine by no means exclude each other, but may be administered to the same patient, either after one another, or combined in such a way that their actions, as regards time, coincide and are conjoined. Indeed, the principal feature in von Liebermeister's method of applying the antipyretic treatment consists not so much in striving to suppress the exacerbations as to prolong and strengthen the remissions of the fever. Antipyretic medicines, he believes, attain their complete value only when they are used in combination with, and as a support to, the cold water treatment. Such a combination produces the strongest remissions, and the most benefit to the patient. And in order to strengthen the reciprocal action of the two, so that their action may correspond with the spontaneous course of the temperature, he holds it requisite to employ the cold bath principally during the night, in order that it also may take effect at the time when the daily temperature is low—i.e., between midnight and morning.

The chapters on Antipyretic Dietetics, and on the Application and Results of the Antipyretic Treatment, will well repay the reader, who cannot fail to be impressed by the calm and admirable manner in which the whole subject is laid before him.

We have left ourselves but little space to speak of Jürgensen's learned and exhaustive essay on Blood-letting and on Transfusion. It constitutes the greater portion of the volume, and is replete with valuable information. Many moot points as to the value or otherwise of transfusion in different cases, and the indications for its performance, are critically discussed. Prof. Jürgensen is not an enthusiast in his recommendation of the operation; on the contrary, he points out that it should never be done in any given case unless a scientifically supported reason can be adduced for its performance as a therapeutic measure.

The remaining portion of the volume is by Prof. Eulenberg, of Griefswald, who is also the author of a classical work on hypodermic injection. In his hands, as might be expected from his high reputation, the subject of the epidermic, endermic, and hypodermic administration of medicines is treated of in a most comprehensive and scientific manner. He also gives numerous directions and hints, based on his large practical experience, which add much to the value of this portion of the work.

2. Dr. Garrod's work is the eleventh edition of what has long been a popular and valued student's text-book. Notwithstanding the number of recent rival works and new editions treating of the same subjects as it does, it holds its position as a useful, matter-of-fact, if not a brilliant companion to the *Pharmacopœia*. Although it does not, as a rule, give so much information as to the physiological action and therapeutic effect of special drugs as some of its contemporaries, its statements, generally, are as complete as the average student requires, or the practitioner looks for. No alteration has been made in the plan of the work, which has been brought into accord with the new *Pharmacopœia*, and edited with care and accuracy by the author's successor in his chair at King's College.

3. In preparing the fourth edition of his "Guide to Therapeutics," Dr. Farquharson has had regard to the very numerous alterations and additions contained in the British *Pharmacopœia* of 1835, so that the work keeps abreast of modern progress. We have still, however, to find fault with the unsystematic way in which the book has been put together, and to the absence of a table of contents which might afford a clue to the plan the author had in view when writing it.

An account of Non-official Preparations, covering some 12 pages towards the end of the work, is very disappointing, from its meagre details, in which respect it contrasts unfavourably with Dr. Whitla's "Manual of Materia Medica and Therapeutics," and with Martin-dale's "Extra-*Pharmacopœia*."

On the whole, the work strikes us as an imitation, on a small scale, of another equally chaotic treatise—Sydney Ringer's "Therapeutics." At the same time, in justice to Dr. Farquharson, we must say that the copious "Index," which closes his book, makes it a handy work of reference to busy practitioners and to students, who prefer to taste, rather than to drink deep of, the Pierian spring.

4. The "Prescriber's *Pharmacopœia*"—a little duodecimo volume, which would almost literally fit into the waistcoat pocket—first saw the light in May, 1841, and we do not wonder that it should have survived nearly half a century and reached a sixth edition. It is thoroughly systematic, and contains an immense amount of information in a remarkably small space.

5. As is stated in the preface of Mr. Semple's handy little book, it consists of an epitome of the new British *Pharmacopœia*, to which



is added a brief statement of the general action of the drugs, and the natural order and chief active principles of those of vegetable origin. It is of convenient size and shape for the pocket, and will be found useful as a handy work of reference by all practitioners desirous of refreshing their memory, and of prescribing in accordance with the Pharmacopœia.

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*The Story of the Heavens.* By ROBERT STAWELL BALL, LL.D., F.R.S., F.R.A.S.; Vice-President of the Royal Irish Academy; Andrews Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland. London, Paris, New York, and Melbourne: Cassell & Co. 1885. 8vo. Pp. 551.

ALTHOUGH this splendid volume is in no sense a medical work, yet we gladly open our columns to a bibliographical notice of it. Written by a distinguished Irishman, the book before us deals with topics of surpassing grandeur and interest connected with the science of astronomy—a science which from the earliest ages has engaged the attention of the mightiest intellects, while many of the phenomena pertaining to it come daily and hourly under the notice of the lowliest and simplest of mankind.

We may be permitted, in the first instance, to congratulate Sir Robert Stawell Ball on the titular dignity which has recently been conferred upon him, and to which he does even more honour than it to him.

The “Story of the Heavens” reads like a romance, far more marvellous than “The Arabian Nights.” As the author says:—“We have indeed a wondrous story to narrate; and could we tell it adequately it would prove of boundless interest and of exquisite beauty. It leads to the contemplation of the mightiest efforts of nature and the greatest achievements of human genius.”

In our opinion, Sir Robert Ball has told the story of the heavens “adequately.” Many of the readers of this Journal are already well acquainted with his descriptive power and command of language. On several occasions he has held large audiences entranced when lecturing at the Royal Dublin Society and in other places on the wonders of astronomy. He possesses the faculty of conveying, even to the unscientific mind, a fair conception of the deepest mysteries of the universe by dressing his thoughts in simplest language, and his powers of simile are unrivalled. An idea of the vast dimensions of the Sun is given by the following

trivial examples:—"If a railway train were laid round the sun, and if we were to start in an express train, moving sixty miles an hour, we should have to travel night and day for five years without intermission before we had accomplished our journey. If the sun be compared with the size of the earth, its stupendous bulk becomes still more apparent. Suppose his globe were cut up into one million parts: each of these parts would appreciably exceed the bulk of our earth. Were the sun placed in one pan of a mighty weighing balance, and were 300,000 bodies as heavy as our earth placed in the other, the sun would still turn the scale."—(Page 26.)

The chapter on the Sun closes with the following eloquent recital of what we enjoy through its benign influence:—"His gracious beams supply the magic power that enables our corn to grow and ripen. It is the heat of the sun which raises water from the ocean in the form of vapour, and then sends down that vapour as rain to refresh the earth and to fill the rivers, which bear our ships down to the ocean. It is the heat of the sun beating on the large continents which gives rise to the breezes and winds that waft our vessels across the deep; and when on a winter's evening we draw around the fire and feel its invigorating rays, we are really only enjoying sunbeams which shone on the earth countless ages ago. The heat in those ancient sunbeams developed the mighty vegetation of the coal epoch, and in the form of coal that heat has slumbered for millions of years, till we now call it again into activity. It is the power of the sun stored up in coal that urges on our steam-engines. It is the light of the sun stored up in coal that beams from every gaslight in our cities. For our power to live and move, for the plenty with which we are surrounded, for the beauty with which nature is adorned, we are immediately indebted to one body in the countless hosts of space, and that body is the sun."—(Pages 47 and 48.)

But it must not be supposed that Sir Robert Ball has sacrificed science on the altar of popular writing. On the contrary, the great scientific truths of astronomy are fully set forth, and the giant discoveries of Kepler, Newton, Copernicus, Huyghens, Galileo, Herschel, and other more modern astronomers, are described and explained.

Among the most interesting chapters in the volume are those on the distant suns (XIX.), in which a comparison between Sirius and our own sun is instituted; the distances of the stars (XXI.),

the spectroscope (XXII.), star clusters and nebulae (XXIII.), astronomical significance of heat (XXV.), and the tides (XXVI.).

The work is splendidly brought out by Messrs. Cassell & Co., and is profusely illustrated with coloured plates and drawings, including pictures of Lord Rosse's telescope, at Parsonstown, and of the great Vienna telescope, recently constructed by Mr. Howard Grubb, of Dublin. We cannot help thinking that such a volume should be in the hands of our professional brethren, and particularly in the hands of those among them whose lot is cast in the open country, and who therefore possess so many favourable opportunities, as they drive by night, of observing the wonders of the starry heavens.

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*Catalogue of the Pathological Museum of the Western Infirmary, Glasgow.* Compiled by JOSEPH COATS, M.D. Glasgow: Printed by Alex. Macdougall, 81 Buchanan-street. 1885. Pp. 244.

THE Museum whose contents are herein catalogued, aims at more than being an appanage of the pathological department. The object of this Catalogue is to make the Museum available as a means of teaching. In composing the descriptions of the specimens catalogued, resort has been had to the Pathological Reports, in which the clinical history has been more or less fully entered. Though such publications possess a paramount local interest, the Series II.—Organs of the Circulation—may be consulted with advantage by any, on account of their very interesting clinical details. The volume is printed in excellent type, and has a useful index.

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*Clinical Figures.* London: Danielsson & Co., 23 Southampton Buildings, Chancery-lane, London, W.C.

THESE figures consist of outlines of bones for general surgery, and also some brain tracings after Feré for marking injuries of that organ. The sheets are gummed, so as to allow of easy transfer to the note-book. They are very useful for every surgeon who carefully records his cases, but it would be an advantage to have the sheets perforated.

# PART III.

## MEDICAL MISCELLANY.

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*Reports, Transactions, and Scientific Intelligence.*

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### ACADEMY OF MEDICINE IN IRELAND.

President—ROBERT M'DONNELL, M.D., F.R.S.

General Secretary—W. THOMSON, M.D.

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#### OBSTETRICAL SECTION.

President—T. MORE MADDEN, M.K.Q.C.P.

Sectional Secretary—WILLIAM C. NEVILLE, M.D.

*Friday, January 8, 1886.*

The PRESIDENT in the Chair.

#### *Specimens Exhibited.*

THE PRESIDENT exhibited a large multilocular ovarian cyst, removed from a multipara, aged fifty, under his care in the Mater Misericordiæ Hospital. The patient had recovered.

MR. J. L. LANE exhibited a so-called hydatid mole, expelled from a patient in the Rotunda Hospital.

DR. MACAN showed:—

(1) An example of commencing carcinoma of the cervix uteri, secondary upon carcinoma of the rectum.

(2) Portions of an intra-uterine fibro-myoma which had undergone myxomatous degeneration. With the aid of Schultze's double forceps-spoon the entire tumour had been successfully enucleated.

(3) A small fibro-myoma removed from the cervical canal by enucleation. The tumour was purely cervical, the internal os being quite closed above it.

(4) A small fibro-angioma which had been attached to the cervix by a pedicle no thicker than a goose-quill.

(5) A papilloma vulvæ of unusual size removed by a ligature round its base.

(6) An ovarian tumour, which had been associated with prolapse of the uterus.

*Displacements of the Ovaries.*

THE PRESIDENT, DR. MORE MADDEN, read a paper upon "Ovarian Displacements." [It will be found in the February Number of this Journal, page 111.]

MR. DOYLE looked upon over-fulness of the intestines as a not uncommon cause of ovarian prolapse. The pressure of the sigmoid flexure upon the left side accounted for the more frequent prolapse on that than upon the right side. Many cases could be cured or relieved by keeping the colon emptied with mercury.

DR. ATTHILL had very seldom found mechanical treatment of any service in cases of prolapsed ovary.

*Fibro-Myomatous Tumour.*

DR. W. J. SMYLY showed a fibro-myomatous tumour which he had enucleated from the cervix of a patient in the City of Dublin Hospital. These tumours occurring in this situation are interesting on account of their rarity, and the obstruction which they are liable to cause during delivery. The operation was simple and the patient made a good recovery. He also exhibited both ovaries and the right Fallopian tube which he had removed from a patient suffering from uterine-fibroids, and whose life was seriously endangered by violent hæmorrhages. The operation was a difficult one owing to the thickness of the abdominal wall and the ovaries being bound down by adhesions, the result of previous perimetritis. The patient made an afebrile convalescence, and has not menstruated since the operation.

DR. MACAN entirely agreed as to the rarity of cervical tumours, and the dangers that might arise from their presence during labour. They might necessitate the destruction of the child, and might subsequently undergo disintegrative changes with accompanying septicæmia. As regards labour, uterine tumours were dangerous in proportion to their nearness to the cervix. In reference to Dr. Smyly's second case, he regarded the removal of the ovaries for bleeding fibroids as one of the greatest improvements in modern gynæcological surgery. Such removal when properly carried out gave certain promise of cure, while the danger of the operation with the aid of antiseptics was almost *nil*. When the tumour was large it might be difficult to rotate it so as to get at the ovaries during the operation, but under any circumstances their removal was much less dangerous than that of the uterus. He would never perform the latter operation when the former was possible.

DR. ATTHILL had formerly relied in cases of bleeding fibroids upon scraping out the uterus and applying nitric acid after the necessary dilatation. Finding, however, that the relief never was permanent he had abandoned this treatment. As a temporising measure he had experienced

better results from the occasional injection of iodised phenol. This might be done once a month where more decisive measures did not seem necessary. Under such treatment he had found the hæmorrhages lessen in amount, and the intervals between them lengthen. Only in a limited number of cases were the pain and hæmorrhage such as to necessitate surgical interference. Enucleation of large tumours was much the most dangerous mode of surgical treatment. When he had formerly tried it he lost patients, and succeeded in removing the tumour only in a few cases. Now he would give the preference to removal of the ovaries, being prepared, in cases where the tumour was large and the ovaries might not be found, to remove the entire uterus. He did not question that palliative measures had formerly been too exclusively relied on.

DR. SMYLY replied briefly.

The Section adjourned.

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## PATHOLOGICAL SECTION.

President—T. EVELYN LITTLE, M.D.

Sectional Secretary—J. B. STORY, M.B.

*Friday, January 15, 1886.*

The PRESIDENT in the Chair.

### *Dentigerous Cyst.*

MR. ARTHUR BAKER read a paper on a case of dentigerous cyst occurring in the lower jaw of a boy aged sixteen. The tumour had existed for six months previous to operation. When opened an unerupted canine tooth was found at the bottom of the cyst. This tooth showed the malformation known as dilaceration of its root. The importance of diagnosing cases of this disease was shown by the fact that in a similar case, where the cyst was not recognised, one-half of the jaw had been excised.

[The paper was illustrated by photographs and casts; the dilacerated tooth was also exhibited.]

MR. STORY asked Mr. Baker did he think dilaceration of the tooth was sufficient to account for the cyst. He stated that there was only one other case on record in which a dilacerated tooth was found occupying the cavity of one of those so-called dentigerous cysts. He (Mr. Story) was of opinion that most of the cases of this sort did not come before the dentist at all, but were seen in hospitals by gentlemen practising surgery.

DR. MACSWINEY said cases of teething were sometimes met with in children, where it seemed impossible for the unaided powers of the child

to erupt the tooth, and on such occasions he had seen a soft tumid condition over the tooth, which had not escaped through the gum, and which when incised with a gum lancet had given exit to a clear viscid fluid. There was in such cases a sort of cyst under the unbroken gum and resting on the crown of the tooth. Was there any connection between those cysts of early primary dentition and the formation of such a cyst as Mr. Baker had described?

DR. CORLEY remarked that the case was an important one for the consideration of all surgeons who were contemplating the capital operation of excision of the lower jaw.

MR. BAKER (in reply) said he did not think the exact cause of such cysts as he had described was known. In the present case the dilaceration was a factor which prevented the eruption of the tooth at the proper time by twisting it out of its proper course. The cysts mentioned by Dr. MacSwiney as occurring over the milk teeth of children were pretty well recognised as of frequent occurrence, and it was undoubtedly a normal condition that a small quantity of fluid should collect between the enamel and the follicle.

*A Case of Perforating Ulcer of the Stomach opening into the Left Ventricle of the Heart and Causing Death by Hæmorrhage, with Exhibition of the Specimen.*

DR. FINNY exhibited an extremely rare specimen of an oval ulcer ( $1 \times \frac{1}{2}$  in.) of the stomach, situated on its anterior wall two and a half inches from the cardiac and two from the lesser curvature, leading up to and perforating the heart. The floor of the ulcer was the muscular structure of the under-surface of the left ventricle, about one inch from the apex; and the ulcer, after perforating the stomach, had eaten through the diaphragm and the pericardium. The stomach was adherent to the diaphragm round the ulcer, but the adhesions were very slight and of recent date. The pericardial sac had been obliterated by adhesive inflammation, and over the whole of the left ventricle the adhesions were very dense. Through the larger of several interstices between the exposed muscular tissue of the apex a probe could be passed upwards, and was found to enter the left ventricle behind a musculus papillaris attached to the posterior mitral curtain.

The patient from whom the morbid specimen was removed had died of syncope on December 9th, 1885, preceded by the passage of blood from the bowels. Strange to say there was no hæmatemesis. At the autopsy the stomach, and the whole intestinal canal, were found full of liquid blood, and the stomach alone contained two quarts. The source of the blood was the left ventricle, and it is probable the blood escaped into the stomach during both systole and diastole.

The clinical history of the patient was as follows:—A farm labourer,

aged nineteen, was admitted under Dr. Finny's care into Sir Patrick Dun's Hospital, at the end of October, 1885, suffering from articular rheumatism of a subacute type, of a month's duration, and presenting the signs of pericarditis in the stage of lymph exudation. In a few days the friction disappeared, while there was no subsequent effusion, and all the other symptoms of rheumatism passed away. There were no symptoms pointing to disease of the stomach, with the exception of pain referable to the sixth rib, about the usual seat of cardiac impulse, and which was also referred to the right coracoid process. This pain was stated to have been present off and on for nearly five years, to be of an intermitting nature, and at times of great severity. During the patient's stay in hospital there were about four such exacerbations, which required morphine for their relief. This pain was never felt in the epigastrium or back; was not induced or relieved by the introduction of food or change of posture; nor did it bring on any nausea or vomiting. From this latter symptom he never suffered, and even during the fatal bleeding there was no hæmatemesis. For seven days before death the pain had greatly abated. During the night of December 8-9 the patient had but little sleep, and in the morning, about 8 a.m., he passed blood from the bowels, and very soon after died in syncope.

The autopsy revealed the conditions described above.

The fatal steps in the pathological history of the case were—1st. Ulcer of the stomach of unknown duration; 2nd. Rheumatic pericarditis and adhesions of the left ventricle to the diaphragm, with obliteration of the sac; 3rd. Recent activity in the ulcer perforating into the muscle of the heart.

The general muscular structure of the heart under the microscope was perfectly normal, and free from fatty degeneration, except the fibres at the floor of the perforation, which were granular and broken down, though free from all fat.

Dr. Finny referred to the anatomical relations of the liver and stomach to the diaphragm, and stated by observations made, that while, in the majority of instances, the liver by its left lobe entirely separates the stomach from the portion of the diaphragm to which the pericardium is applied, there are exceptional specimens where the left lobe of the liver is small and pointed, where a small triangular portion of the extreme front of the pericardial aspect exactly corresponds to the anterior wall of the stomach in the very position of the ulcer present in this case. This he further illustrated by reference to Braune's topographical plate taken from the frozen subject. Dr. Finny pointed out the extreme rarity of such cases as his, and when he first discovered its peculiar features he thought it was unique, inasmuch as no such specimen was to be found in the pathological museums of Dublin or London, and no record of such a case was to be met with in the "*Pathological Transactions*," or in any of



the books devoted to the subject of gastric ulcer, nor could he obtain any reference to any reported instance in the medical literature of Great Britain and Ireland.

Further research, however, showed him that it was not unique, as there are three similar cases recorded in Vienna—the first by H. Chiari in 1880, the second by F. Brenner, and the third by Oser in 1881; and each of these latter observers, like Dr. Finny, seemed to have been unaware of the other reported cases.

DR. FRAZER said he believed this case was altogether novel in Dublin. He had seen two cases of gastric ulcer, both of which occurred in young females, and were attended by rupture. In both the patients survived for a time—one for some years. He was enabled, by the recurrence of the rupture in both cases afterwards, and by circumstances brought under his notice after death, to verify the fact that the ulcers had perforated the stomach and had afterwards healed. The nearest approach to the case of Dr. Finny was one that occurred in the practice of Dr. M'Dowel, where an abscess of the liver penetrated into the pericardium and set up inflammation, and air got into both stomach and pericardium. He did not know whether or not the case was recorded in the "Transactions of the Pathological Society;" but he remembered seeing it, and there were pericardial friction sounds showing that it was an abscess of the liver which had burst. Pain recurred at intervals, and was only relieved by the patient lying on his back. Some years ago a medical man had an ulcer in the same part of his stomach, but in his case the pain was only relieved by strong pressure over the stomach. He wore a pad and had to lie on his face in order to relieve the pain, which was extreme.

DR. DUFFY said he had observed a case of ulcer of the stomach in which an opening was formed into the lungs, causing a gangrenous abscess. He presented the specimen to the Museum, and it was now on the table of the College of Surgeons. The patient was a young woman, twenty years of age, who, after an illness of 64 days, which commenced with a pleuritic attack, developed symptoms resembling those of enteric fever. She died with symptoms of gangrene of the lung; and after her death, in the lower lobe of her left lung was found a gangrenous abscess the size of a man's fist, communicating with a small ulcer in the anterior wall of the stomach. There were no symptoms of gastric ulcer that he could recognise during life. The base of that ulcer was in close proximity to the left ventricle of the heart, but had not perforated it. The case reported by Professor Chiari occurred in a woman seventy-one years old. That of Professor Oser was in a woman of the same age. Brenner's case occurred in a woman of fifty-five. In Chiari's case the patient had symptoms of hæmatemesis and passage of blood from the intestines. There was a round hole, the full report stated, two centimetres in diameter, in the lesser curvature of the stomach, leading into a sac as

large as a walnut, which reached the left ventricle and presented an ulcerous opening. The heart was adherent to the pericardium, and there was fatty degeneration of the heart. In those three cases there were vomiting of blood and tarry stools. With respect to Dr. Finny's case, it had occurred to him that the perforating ulcer might have set up sufficient irritation in the diaphragm to cause adhesion between the adherent pericardium and the diaphragm, and the subsequent perforation. He did not think that the gastric juice could have had so much influence as Dr. Finny attributed to it. A feature in many cases of gastric ulcer was the great latency of the symptoms.

DR. HENRY KENNEDY said, as regarded pain, a great deal depended on the situation of the ulcer. According to his experience, when the ulcer was on the anterior wall of the stomach, the suffering was very much less than in other cases, and was also intermitting. On the other hand, when the ulcer was down near the pylorus, the pain was nearly constant on the occasions of taking food. He had met with cases in which the patients were only relieved by lying on their faces. When they went to sleep in the ordinary position they would awake finding themselves lying on their faces, as if, on the occurrence of irritation, a sort of instinct had forced them to assume the easiest posture.

The PRESIDENT remarked that Dr. Finny, speaking of the manner in which the ulcer, after perforating and causing an adhesion of the pericardium, made its way to the ventricle of the heart, laid stress on what he called solution by the gastric juice of the muscular wall of the heart. That seemed rather a startling novelty in pathology, and he should prefer to arrive at some other explanation of the fact. The idea of solution by gastric juice was suggested by Chiari's case, in which it was stated that the muscular fibre of the heart round the situation where the rupture occurred was in a condition of fatty degeneration. Unless there was some very good evidence that the solution in Dr. Finny's case was caused by the gastric juice he would incline to think that some form of degeneration was present also. Hardly any part of the abdominal viscera was subject to so much irregularity, as to both position, size, and shape, as the left lobe of the liver. Therefore, what had occurred in Dr. Finny's case did not present any great difficulty to his mind.

DR. DUFFEY said there was one circumstance mentioned in Chiari's case, which, although no weight was attached to it by the reporter, might be of importance—namely, that in the wall of the sac close to the opening into the stomach a hard brittle mass was found, which chemical examination proved to be composed chiefly of silicates, and which was probably a piece of glass that had been accidentally swallowed.

DR. MACSWINEY said that solution of tissue by gastric juice was only possible after there had been necrosis of the tissue. In Dr. Finny's case a portion of the pericardium might have become dead tissue.

DR. FINNY, in reply, said in his case the specimen contained no foreign body. He thought the fact of a chamber having been formed between the heart and the pericardium showed that probably a gastric ulcer first formed the abscess, and that it opened from the stomach, forming a smaller one underneath the diaphragm, and from thence reached the pericardium. Having regard to the slight character of the adhesion between the outer surface of the stomach and the diaphragm, he did not think that the passing of the ulcer through the diaphragm could have set up such pericarditis as was found in the case. He thought that pericarditis was due to a rheumatic attack which had preceded the perforation of the diaphragm by the ulcer from the stomach. Nor did he see any difficulty about the solution of the muscles of the heart by the gastric juice. Dr. Purser and Dr. Bewley, who had examined the floor of the ulcer and the granular muscular tissue of the heart, would bear him out in the statement that not the least appearance of fatty degeneration was exhibited by the portions of those parts that were examined under the microscope. The base of the ulcer and the muscular tissue of the heart were turned into a granular *débris*, which was the result of the solution. The rest of the heart was perfectly healthy, and showed no sign of fatty degeneration whatever. Nor did he see why the heart should not have suffered from the action of the gastric juice, when they saw that the tendinous portion of the diaphragm, which opposed the progress of the ulcer, was dissolved by the gastric juice at the floor of the ulcer. Having thus gone through the diaphragm, which was of its full thickness, the gastric juice might have acted in a similar way on the muscular tissue of the heart.

*Specimens of Bright's Disease.* By DR. A. W. FOOT.

These kidneys are representative examples of the last stage of parenchymatous nephritis when the organs have undergone atrophic changes. They are the variety of Bright's kidney, called by G. Johnson the "small fatty kidney."

They are symmetrically affected, are both reduced in size, weighing together  $6\frac{1}{2}$  oz. (about half their normal weight). The pallid surface is marbled with pale yellow, smooth areas, defined by surrounding pinkish, because more vascular districts. Several cysts are observed over their exterior. They have not the red brown colour, the roughness, toughness, or diminished cortex which are obvious features in the small contracted kidney or "cirrhosis" of interstitial nephritis.

The heart presents the appearances of a notable, though not extreme, degree of hypertrophy of the left ventricle without valvular disease or atheroma in the aorta. The hypertrophy was becoming changed from the simple into the eccentric form, because already the ventricle is dilated beyond its usual dimension. The pericardium was universally adherent, but had recently become so as it could be peeled off without

any great difficulty. The heart, empty of coagula, freed from the pericardium and any superfluous parts of its large vessels, weighed 16 oz.—about double its usual weight. The lungs presented the features of œdema and capillary bronchitis.

The specimens were obtained from the body of a labouring man of thirty-three years of age, who had been habitually exposed to the cold and moisture of the Wicklow Mountains, in which he lived. He was admitted into my wards in the Meath Hospital, 1st December, 1885, at which time he had the sallow pallor of Bright's disease. His urine was loaded with albumen. He had ascites and general anasarca, and a copious purulent expectoration from the bronchial tubes. He dated his illness from cold four or five months previously, which had been quickly followed by œdema of the face, legs, and genital organs. Since this cold he had frequently had epistaxis and cramps in the legs and forearms. Immediately after admission the total urine of twenty-four hours was collected, and amounted to but 38 fl. oz., clear, pale, 1011 sp. grav. The quantity of urea in it was 5.53 parts per 1,000. The total quantity of urea in the 38 fl. oz. was 91.87 grains. Next day (5th Dec.) the urinous odour of his breath was very perceptible. In a few days diarrhœa came on, from six to eight motions in the twenty-four hours; getting rid, probably, of some of the urea the kidneys failed to eliminate. No effect was produced upon the dropsy by rest in bed, milk diet, hot air and hot water baths given on alternate days. *His sight he made no complaint of,* but said, in answer to inquiries, it was "as good as ever." He died quietly on the 20th December.

It was remarkable in this case to what a degree the man preserved his appetite. In answer to inquiries on the subject, he said he "could eat anything at all." Bartels has observed this matter, and how, even with this capacity for food, the anæmia, emaciation and loss of strength increase uninterruptedly. This fact, he remarks, should not cause astonishment, when we bear in mind the enormous quantities of albumen that are eliminated with the urine in this disease. The hypertrophy of the left ventricle in this case is attributable to the secondary atrophy of the kidneys; it is the consequence of the process of contraction, and is invariably absent in the bodies of those who succumb at the height of the chronic nephritis.

DR. HENRY KENNEDY said that in the cases now before the Section it was mentioned that there had been adhesions to the pericardium. Some years ago he inquired into this point, and found that in the bulk of the cases in which there were adhesions the heart was enlarged. In some cases that he had seen of Bright's disease albumen disappeared altogether from the urine towards the end.

DR. MACSWINEY asked could Dr. Foot give any explanation of contracted kidneys occurring in this man, who was a young countryman,

only thirty-three years of age, and with respect to whom there was no history of gout, or rheumatism, or syphilis, or drink, or lead. One circumstance in the present case was not in accordance with what was generally met with in cases of chronic, contracted, or gouty kidneys—namely, the urine was clear, its specific gravity low, the albumen abundant, and the urea rather scanty.

Dr. Foot (in reply) said he did not think the adherent pericardium had much to do with the hypertrophy in the case, because of the effectiveness of the valves. Dr. MacSwiney had misunderstood the kind of kidneys these were. He had distinctly stated that they were not the small, red, tough, rough, contracted kidney of cirrhosis or interstitial nephritis, but were an advanced stage of the large white kidney. They were the small white kidney into which the large white kidney turned, and in which the urea was scanty and the albumen copious—just the reverse of what was the case in the sclerotic kidney. The man was exposed to causes which the ætiologist looked on as the most reliable for the production of the disease. He lived in a wretched cabin on a mountain side, and was habitually exposed to cold and moisture. He said that he took a “sup of drink” whenever he could get it, but that the occasions were rare.

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## MEDICAL SECTION.

President—F. R. CRUISE, M.D.; President and Fellow, King and Queen's College of Physicians.

Sectional Secretary—A. N. MONTGOMERY, M.K.Q.C.P.

*Friday, January 29, 1886.*

Dr. FINNY in the Chair.

### *Therapeutic Uses of the Digestive Ferments.*

Dr. PURSER read a paper on the above subject. [It will be found at page 193.]

Dr. HENRY KENNEDY took exception to Dr. Purser's observations about the effect of pepsine, of which he had had favourable experience himself in children with whom imagination could not be said to operate. How pepsine acted he was unable to explain. Some preparations of pepsine with glycerine were calculated to develop an unpleasant smell, which made it disagree.

Dr. FALKINER remarked that when he was at Hamilton and Long's there were 65 gallons of the preparation made and prescribed in Dublin within a year, and some of Long's pepsine after eight years was as good as the day it was made.

Dr. WALTER G. SMITH regarded Dr. Purser's conclusions as founded

on uncontrovertible data. It was high time to protest against the illogical and irrational mode of using so-called aids to digestion, which were sent abroad wholesale under the guise of scientific foundation. The earliest preparation of the article was one that made some stir—namely, pepsine wine, which was devoid of any digestive powers, and was driven from the field by more active advertising. The real uses of ferments were clearly indicated by Dr. Purser, and were those to which many practitioners restricted themselves—namely, the preparation of food before it was put into the patient's stomach or into the rectum. One of the most important preparations was that of nutritive enemata.

DR. J. W. MOORE said that rectal alimentation was one of the principal uses of digestive agents, from the addition of which to nutritive enemata he had seen good results.

DR. ATTHILL said he was sure Dr. Purser's remarks were based on scientific principles, but possibly some of his deductions were not strictly correct; because experiments carried on outside the body could not be identical with experiments inside. While sensible that there was a vast amount of humbug in the sale of digestive ferments, he did not agree with Dr. Purser that those ferments were absolutely useless, having himself prescribed pepsine with good results, especially in the case of children and delicate women. In artificial feeding *per anum* the peptonised food was of the greatest importance. He had performed ovariectomy seventy times, and in cases of vomiting, where artificial feeding *per rectum* became necessary, he formerly found the only food absorbed in that way was cold beef tea and alcohol.

The CHAIRMAN (DR. FINNY) concurred, from his own experience, with Drs. Kennedy and Atthill as to the value of the therapeutic influence of pepsine, which he believed to be of great use in imperfect digestion, in spite of the chemical and physiological reasons brought forward against it by Dr. Purser. To administer drugs upon the principle of strict adherence to chemical reasons would lead to their giving up the administration of drugs altogether. He had found great advantage from the liquor pepsine where the gastric digestion was delayed, and hence he protested against Dr. Purser's statement that pepsine employed in the ordinary way was an utterly useless ingredient.

DR. PURSER replied.—The method of extracting digestive ferments with glycerine was very well known, glycerine being one of the most powerful solvents. He was prepared for the protest made as to the use of pepsine, and had anticipated the objection in his paper. When a student, with more faith in drugs than now, he took pepsine and could not say it did him the smallest particle of good. As to the effect of medicine on children and delicate women, he did not say the effect was produced through the imagination; but he did not know anybody who gave pepsine and nothing else. The diet was always regulated to make

it more easily digestive, and pepsine was prescribed with acid or aromatic water, and aromatic water would often cure gripes in children. To say the thing was different inside the body and outside the body showed an erroneous conception of digestion which did not take place inside but outside the body. The mucous membrane of the stomach was just as much outside the body as the palm of the hand. The preparations were of some value for rectal alimentation.

*Fætid Expectoration from the Lung.*

DR. H. KENNEDY detailed two instances, both in young females, where a very profuse expectoration, attended by a most offensive odour, occurred. The fætor was so great as to diffuse itself through a large ward, like what occurs in gangrene of the lung. The physical signs were in each case confined to one lung, and were due to what is now known as chronic strumous pneumonia. In each case there were slight signs of hectic, with the nails curved; whilst menstruation was irregular. Under the use of a combination of powdered uva ursi and charcoal the patients improved much in their general health, and the fætor quite ceased at the end of ten days, and in about a month each patient left hospital. No local treatment in the way of inhalation was used.

DR. R. A. HAYES instanced the case of a woman in Dr. Steevens' Hospital whom he had seen at the request of a colleague; the patient was the subject of a fætid bronchitis. He suggested an inhalation consisting of a combination of creasote, carbolic acid, iodine, and spirit, and in twenty-four hours the smell decreased and disappeared entirely in a few days.

DR. WALTER G. SMITH said he had seen several cases of pulmonary gangrene or abscess which was produced often by putrefactive bacteria in the air passages, irrespectively of the lung. The use of charcoal had been over-estimated. It would absorb gases, fætid odours, and abstract alkaloids in the dry state, but once thoroughly wet its deodorising qualities ceased. The distance to which drugs penetrate by inhalation into the air passages was much less than was generally supposed, it being doubtful if they get beyond the trachea, much less into the air passages and lungs. He doubted that the smell of gangrene of the lung could be diminished in that way, and said the rational plan was to give plenty of fresh air.

DR. J. W. MOORE, MR. DOYLE, and MR. FOY, also took part in the discussion, and

DR. H. KENNEDY replied.—The treatment suggested by Dr. Hayes would of course now be first adopted. He had seen marked improvement from charcoal treatment—the charcoal being given with water or in biscuits, five grains in each biscuit. The uva ursi was a good medicine and was inexpensive. It affected ordinary bronchitis, and had been given a century ago in cases of decline.

## CLINICAL RECORDS.

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*Case of Suicide by Butting the Head against a Stone Wall.* By WILLIAM ALLAN, J.P., L.R.C.S.I., L.M., F.R.G.S., &c.; Assistant Colonial Surgeon, Bathurst, Gambia, &c., &c.

BURAY SENGORE, an adult male black, undergoing imprisonment with hard labour in the gaol of Bathurst, for larceny, committed suicide in the following manner, in the presence of witnesses, on the morning of the 3rd of December, 1885. He had been an inmate of the gaol from the 13th of July, 1885, and had not during that period exhibited any symptoms of insanity.

The case has, in my opinion, much interest in a medico-legal point of view, as if he had not been seen by witnesses to commit suicide it might have been reasonably supposed that—on the body being found lying with a lacerated wound on the scalp and the bones of the head fractured—he might have been struck by a prison-warder or a fellow-convict. There was no scaffolding work going on in the gaol at the time, so this fact does away with any idea that he had fallen from a height, and so received injury.

The history of the case is as follows:—On the morning of the 3rd of December he took his breakfast as usual. After eating, he sat down in the yard behind the kitchen. That morning his wife had been brought into the gaol as a prisoner convicted for larceny; he saw her, and a fellow-convict said to him, "Look, your wife has come." This circumstance seemed to have acted on his mind, for, on being summoned for labour duty, he did not respond at once, and on being spoken to again he started off at full speed towards the latrines, built of stone, and dashed the vertex of his head against the building. So great was the concussion that he dropped dead without a struggle. The distance run would be about fifty feet. A mark where his head struck was made on the wall, but no stain of blood. A small quantity of blood flowed from the scalp wound, and about ten minutes after copious hæmorrhage took place from the nostrils. The body rebounded some three feet from the wall. He was a tall man and of spare build.

I have examined the skull, and found injuries as follow:—Lambdoidal suture intact, but nearly two-thirds of the sagittal suture open, and, where it joins the coronal, is a small fracture of the right parietal bone. The whole of the left coronal suture was freely opened, the right intact. The frontal bone was fractured for a little more than one inch right through the bone. The corresponding parts of the membranes and the brain itself were lacerated.



# SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P., F. R. Met. Soc.

## VITAL STATISTICS

*For four Weeks ending Saturday, January 30, 1886.*

The deaths registered in each of the four weeks in the sixteen principal Town Districts of Ireland, alphabetically arranged, corresponded to the following annual rates per 1,000 :—

TOWNS	Weeks ending				TOWNS	Weeks ending			
	Jan. 9.	Jan. 16.	Jan. 23.	Jan. 30.		Jan. 9.	Jan. 16.	Jan. 23.	Jan. 30.
Armagh -	15·5	5·2	25·8	20·7	Limerick -	33·7	29·7	22·9	32·4
Belfast -	25·6	29·4	25·6	24·0	Lisburn -	19·3	29·0	38·7	24·2
Cork -	18·8	31·2	16·2	35·0	Londonderry	28·5	23·2	26·7	19·6
Drogheda	12·7	29·6	21·1	4·2	Lurgan -	25·7	41·0	25·7	41·0
Dublin -	27·6	32·3	29·2	30·6	Newry -	10·5	38·6	21·1	24·6
Dundalk -	17·5	17·5	26·2	17·5	Sligo -	9·6	24·1	4·8	19·2
Galway -	26·9	23·5	30·3	37·0	Waterford -	37·0	25·5	18·5	44·0
Kilkenny	16·9	12·7	46·5	29·6	Wexford -	38·5	42·8	42·8	12·8

In the week ending Saturday, January 9, the mortality in twenty-eight large English towns, including London (in which the rate was 22·4), was equal to an average annual death-rate of 22·5 per 1,000 persons living; in Glasgow the rate was 28·7; and in Edinburgh 16·1. The average annual death-rate represented by the deaths registered in the sixteen principal town districts of Ireland was 25·7 per 1,000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 2·0 per 1,000, the rates varying from 0·0 in Limerick, Newry, Kilkenny, Drogheda, Wexford, Dundalk, Lisburn, and Armagh, to 5·1 in Lurgan; the 5 deaths from all causes registered in the last-named district comprising 1 from measles. Among the 109 deaths from all causes registered in Belfast are 1 from measles, 3 from scarlatina, 2 from whooping-cough, 1 from diphtheria, 1 from simple continued fever, and 1 from diarrhoea; and the 29 deaths in Cork com-

prise 1 from measles, 4 from scarlatina, and 1 from typhus. One of the 2 deaths registered in Sligo was caused by typhus.

In the Dublin Registration District the births registered during the week amounted to 195—109 boys and 86 girls, and the deaths to 195—91 males and 104 females.

The deaths represent an annual rate of mortality of 28·8 in every 1,000 of the population, estimated to the middle of the present year; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 27·6 per 1,000.

Nineteen deaths from zymotic diseases were registered, being 20 under the number for the preceding week, and 13 below the average for the first week of the last ten years; they comprise 3 from scarlet fever (scarlatina), 1 from typhus, 8 from whooping-cough, 1 from ill-defined fever, 1 from enteric fever, 1 from diarrhœa, &c.

In the week ending Saturday, January 16, the mortality in twenty-eight large English towns, including London (in which the rate was 23·9), was equal to an average annual death-rate of 23·8 per 1,000 persons living; in Glasgow the rate was 26·5; and in Edinburgh 22·1. The average annual death-rate in the sixteen principal town districts of Ireland was 30·1 per 1,000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 3·4 per 1,000, the rates varying from 0·0 in ten of the districts to 10·5 in Newry; the 11 deaths from all causes registered in that district comprising 2 from whooping-cough and 1 from diarrhœa. Among the 125 deaths from all causes registered in Belfast are 3 from measles, 3 from scarlatina, 3 from whooping-cough, 2 from diphtheria, 3 from enteric fever, and 1 from diarrhœa; and the 48 deaths in Cork comprise 1 from measles, 1 from scarlatina, 3 from whooping-cough, and 1 from diarrhœa.

In the Dublin Registration District the births registered during the week amounted to 186—101 boys and 85 girls, and the deaths to 225—118 males and 107 females.

The deaths represent an annual rate of mortality of 33·2 in every 1,000 of the estimated population; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 32·3 per 1,000.

Thirty-two deaths from zymotic diseases were registered, being 13 over the number for the preceding week, but 3 under the average for the second week of the last ten years; they comprise 4 from scarlet fever (scarlatina), 19 from whooping-cough, 4 from enteric fever, 3 from diarrhœa, &c.

In the week ending Saturday, January 23, the mortality in twenty-eight large English towns, including London (in which the rate was

21·9), was equal to an average annual death-rate of 22·4 per 1,000 persons living; in Glasgow the rate was 28·8; and in Edinburgh 19·2. The average annual death-rate in the sixteen principal town districts of Ireland was 26·4 per 1000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 2·7 per 1,000, the rates varying from 0·0 in ten of the districts to 5·3 in Londonderry; the 15 deaths from all causes registered in that district comprising 2 from whooping-cough and 1 from enteric fever. Among the 109 deaths from all causes registered in Belfast are 3 from measles, 8 from scarlatina, 1 from ill-defined fever, 1 from enteric fever, and 4 from diarrhoea; and the 25 deaths in Cork comprise 1 from each of the following diseases:—scarlatina, typhus, and diarrhoea.

In the Dublin Registration District the births registered during the week amounted to 168—91 boys and 77 girls, and the deaths to 200—95 males and 105 females.

The deaths represent an annual rate of mortality of 29·5 in every 1,000 of the estimated population; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 29·2 per 1,000.

Twenty-seven deaths from zymotic diseases were registered, being 5 under the number for the preceding week, and 8 under the average for the third week of the last ten years; they comprise 2 from measles, 2 from scarlet fever (scarlatina), 17 from whooping-cough, 1 from cerebro-spinal fever, 2 from enteric fever, &c.

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In the week ending Saturday, January 30, the mortality in twenty-eight large English towns, including London (in which the rate was 22·3), was equal to an average annual death-rate of 22·1 per 1,000 persons living; in Glasgow the rate was 26·5; and in Edinburgh 16·5. The average annual death-rate in the sixteen principal town districts of Ireland was 28·4 per 1,000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 1·9 per 1,000, the rates varying from 0·0 in Londonderry, Newry, Drogheda, Wexford, Dundalk, Sligo, Lisburn, Lurgan, and Armagh, to 6·7 in Galway; the 11 deaths from all causes registered in the last-named district comprising 1 from whooping-cough and 1 from enteric fever. Among the 102 deaths from all causes registered in Belfast are 3 from scarlatina, 3 from whooping-cough, 1 from enteric fever, and 1 from diarrhoea.

In the Dublin Registration District the births registered during the week amounted to 187—93 boys and 94 girls, and the deaths to 216—3 males and 93 females.

The deaths represent an annual rate of mortality of 31·9 in every 1,000

of the estimated population; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 30·6 per 1,000.

Twenty-one deaths from zymotic diseases were registered, being 14 below the average for the corresponding week of the last ten years, and 6 under the number for the week ended January 23rd; they comprise 1 from scarlet fever (scarlatina), 1 from typhus, 13 from whooping-cough, and 3 from cerebro-spinal fever.

### METEOROLOGY.

*Abstract of Observations made in the City of Dublin, Lat. 53° 20' N., Long. 6° 15' W., for the Month of January, 1886.*

Mean Height of Barometer,	-	-	-	29·662 inches.
Maximal Height of Barometer (on 11th, at 9 p.m.) -	-	-	-	30·287 „
Minimal Height of Barometer (on 17th, at 9 p.m.),	-	-	-	29·032 „
Mean Dry-bulb Temperature,	-	-	-	37·6°.
Mean Wet-bulb Temperature,	-	-	-	36·1°.
Mean Dew-point Temperature,	-	-	-	33·9°.
Mean Elastic Force (Tension) of Aqueous Vapour, -	-	-	-	·198 inch.
Mean Humidity, -	-	-	-	86·9 per cent.
Highest Temperature in Shade (on 3rd), -	-	-	-	53·4°.
Lowest Temperature in Shade (on 7th), -	-	-	-	24·9°.
Lowest Temperature on Grass (Radiation) (on 7th), -	-	-	-	20·0°.
Mean Amount of Cloud, -	-	-	-	65·7 per cent.
Rainfall (on 26 days), -	-	-	-	3·244 inches.
Greatest Daily Rainfall (on 25th), -	-	-	-	·901 inch.
General Directions of Wind, -	-	-	-	W., S.W., N.W.

### Remarks.

Although the New Year opened with very mild, dull weather, January, 1886, will be remembered as a cold, snowy month. It is true that no severe frosts were recorded near Dublin, but in many parts of the United Kingdom extremely low temperatures were noted from time to time—the lowest of all being 7° Fahr. at Newton Reigny, near Penrith, in Cumberland, on the 19th, and 6° at Rothamsted, on the 8th. Some idea of the severity of the weather may be gathered from the fact that in Dublin snow or sleet fell on eighteen days, and hail on fourteen days. Both the rainfall and rainy days were much above the average, while atmospheric pressure and temperature were as much below it. The maximal temperature did not exceed 40° from the 17th to the 27th inclusive.

The mean height of the barometer was 29·662 inches, or 0·219 inch below the average value for January—namely, 29·881 inches. The mercury rose to 30·287 inches at 9 p.m. of the 11th, and sank to 29·032 inches at 9 p.m. of the 17th. The observed range of atmos-

pherical pressure was, therefore, 1·255 inches—slightly more than an inch and a quarter. The mean temperature deduced from daily readings of the dry bulb thermometer at 9 a.m. and 9 p.m. was 37·6°, or 4·3° below the value for December, 1885; that calculated by Kaemtz's formula—viz.,  $\text{min.} + (\text{max.} - \text{min.} \times \cdot 41) = \text{Mean Temp.}$ —from the means of the daily maxima and minima was 37·1°, or 3·7° below the average mean temperature for January, calculated in the same way, in the twenty years, 1865–84, inclusive (40·8°). The arithmetical mean of the maximal and minimal readings was 37·9°, compared with a twenty years' average of 41·4°. On the 3rd the thermometer in the screen rose to 53·4°—wind S.W.; on the 7th the temperature fell to 24·9°—wind W. The minimum on the grass was 20·0° on the same date. The diurnal range of temperature was considerable on the 3rd, and from the 10th to the 15th inclusive, but the maxima in this period were sometimes reached at night. The rainfall was 3·244 inches, distributed over 26 days. The average rainfall for January in the twenty years, 1865–84, inclusive, was 2·243 inches, and the average number of rainy days was 17·1. The rainfall, therefore, and the rainy days were both decidedly above the average.

Sleet or snow fell on the 5th and four following days, the 13th, the 15th and six following days, the 23rd, 24th, 25th, 30th and 31st—in all on eighteen days. Hail was noted on the 6th, 8th, 13th, 16th and four following days, the 22nd and three following days, the 30th and 31st—in all on fourteen days. Ice-crystals were seen on the evening of the 19th. The air was more or less foggy on the 7th, 10th, 22nd, 25th, 26th, 27th, and 28th. Lunar halos were observed on the nights of the 20th and 22nd, and a solar halo appeared on the 28th. High winds prevailed on thirteen days.

At the beginning of the year temperature was generally high for the season.

In the course of the week ending Saturday, the 9th, the dominant systems of atmospherical pressure over the United Kingdom were alternately cyclonic and anti-cyclonic, the wind-type varying from south-westerly to north-westerly or northerly. The weather was very changeable—at times mild and again very cold and foggy. On the 6th a heavy snowstorm prevailed in the valley of the Thames.

During the second week the weather was most changeable. The distribution of pressure was cyclonic—deep depressions appearing in the far north, while elongated subsidiary disturbances travelled in an easterly direction across our Islands. Southerly winds, rain and comparatively mild weather alternated with north-westerly and northerly winds, snow, sleet, hail and frost—the changes from one type to the other being sudden and frequent.

In the third week (ending the 23rd), pressure distribution was again

mainly cyclonic, but the type was complex and variable. On Monday, the 18th, the barometer was very low over the North Sea and so continued for some days. Shallow depressions then began to advance *westwards* from Germany to the S.E. of England—cold weather was thus continued, with frequent falls of snow or sleet in many places, and occasionally clearer skies and frost.

The weather remained very unsettled and changeable in the fourth and last week. On the 24th and following days a depression lay over the Bay of Biscay, Brittany, the English and St. George's Channels. Heavy snow fell in and about Dublin on Sunday and Monday, the 24th and 25th, as well as in the S.W. of England. At 9 a.m. of the 25th the temperature in Dublin was  $27.2^{\circ}$ , and the appearance was quite Arctic. A thaw followed in the afternoon and a heavy fall of rain took place at night. The low pressure system in the S. gradually filled up, but on the 28th the barometer fell to the W. and N.W., and a new series of depressions began to come in from the Atlantic, causing showery, broken weather to the end of the month.

It is noteworthy that the cold weather appeared to be of Atlantic or North American origin—it certainly came in from the westward, at a time when no extreme cold prevailed in the N. or N.W. of Europe; and it also showed itself near the centre of an extensive and very deep atmospherical depression.

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## PERISCOPE.

### THE CAUSE OF THE FIRST SOUND OF THE HEART.

An interesting note on the cause of the first cardiac sound, by Dr. Gerald Yeo and Dr. J. W. Barrett, has appeared in the *Journal of Physiology*. Opinions, as is well known, have varied considerably on this point. Some observers, as Halford and Billing, looking at the relative size of the auriculo-ventricular and semilunar valves, have held that the sudden tension of the former is sufficient to produce the first sound of the heart. These observers point to the fact that just as the act of hooking back one semilunar valve abolishes the second sound, so the same act of hooking back one auricular valve, or the incompetence of the valve as a whole, impairs or abolishes the first sound; and they also point to the fact that the tracing of a cardiac contraction is a single contraction, and not a tetanus of the muscular tissue. Many careful experimenters, however, maintain that the muscular sound must be regarded as an element in its causation, and insist on the greatly increased volume of the sound in cases of cardiac hypertrophy. Drs. Yeo's and Barrett's experiments originated in a difference of opinion between them on this point, each being desirous of persuading the other of the soundness of his view.

To determine the question, a large cat and an active mongrel bull-terrier were chloroformed and subjected to artificial respiration, and the cardiac sounds were then carefully listened for by each disputant, as well as by some independent observers, after the thorax had been opened by an extensive median incision without injury to the pericardium. The veins were then compressed, and all noted that the sound became gradually slightly diminished, but did not become inaudible, the tone remaining distinct as long as the heart continued to beat. In the case of the dog, the same phenomenon was observed even after the heart was removed from the body, and the same was noted in the ventricle when removed below the valves. The authors therefore arrived at the conclusion that a definite and characteristic tone, similar in quality to the first sound, is produced by the heart-muscle under circumstances that render it impossible for any tension of the valves to contribute to its production.—*Lancet*, Oct. 31, 1885, and *Practitioner*, Feb., 1886.

#### AN UNUSUAL ACCIDENT.

MR. CHARLES TOMES, M.A., F.R.S., reports the case of a patient, aged about twenty-five, who recently presented himself with the intention of having the right upper lateral incisor pivoted. A week previously he had had a severe fall in the hunting field, with the result of loosening the upper and lower centrals and breaking off the lateral incisor short. On examination, however, the lateral incisor was found not to be broken at all, except that the edge was to a trivial extent chipped; but it had been driven up into its socket vertically until its cutting edge nearly corresponded with the level of the gum. Very little pain had been experienced, and when the patient was seen there was only the least trace of inflammation; the displaced tooth was so exceedingly firmly wedged into its new position that not the least motion could be detected when it was pressed upon in any direction. As it was perfectly useless in the position occupied, Mr. Tomes decided to attempt to draw it down, and, with this object, took an impression, and made a small vulcanite splint, which was to be tied on to the bicuspid on each side, and which had holes to ligature the lateral, if it could be brought down. In order to enable him to deliberate in his manipulation, the patient was placed under gas, and the tooth grasped with thin-bladed stump forceps, the blades of which had been dipped in eucalyptus oil; the tooth required as much or more force than would ordinarily suffice for the extraction of a lateral before it could be stirred in the smallest degree, and when it moved from its bed it came out at once. The apex of the root, however, barely left the socket, and it was instantly replaced, and forcible pressure made upon the gum over the socket on both the labial and parietal aspects. The gum readily squeezed down on the root, yielding the sensation of there being no bone at all under the finger.

The tooth was ligatured to the splint, and has since done well, being quite free from tenderness after the first two days, and firm enough to dispense with the splint on the fourth day. The principal question to be settled, which, of course, was considered beforehand, was what plan should be adopted if it came out, which was obviously the most likely thing to happen. The choice lay between its instant replacement or the removal of its pulp and the filling of its roots prior to its replacement. Mr. Tomes decided in favour of instant replacement, because it is well known that teeth immediately replanted generally do very well, notwithstanding that they contain dead pulps, and in this case the complete breaking up of its original socket interposed considerable difficulties in the way of its becoming fixed. The pulp cavity Mr. Tomes proposes to open up and fill after the healing of the socket is presumably complete. The risk of abscess is, judging by other cases, by no means great, nor is it immediate, and the objection that the tooth may become discoloured by leaving its pulp in for a time has far less force, in Mr. Tomes' opinion, than it would otherwise have had, in consequence of a case which he treated some years ago, in which a boy knocked out his central incisor, carried it in a dirty pocket for twelve hours, and brought it to him begrimed. In this case he enlarged the apical foramen, removed the pulp entire, and filled with oxychloride of zinc, closing the foramen with gold. But though the tooth united, and has done perfectly well ever since, its colour is not satisfactory; it has the characteristic colour of a dead tooth, so that the complete removal of the pulp has not been attended with the advantage as to colour which would, *a priori*, have been expected.—*Journal of the British Dental Association*.

#### URETHAN.

DR. A. S. MYRTLE, of Harrogate, writing of this new hypnotic, says:— Since October I have been using urethan in a variety of cases with satisfactory results. I have used it in over fifty cases as a sedative and hypnotic, and my experience of its action encourages me to recommend the drug, believing that, in certain cases, it will prove of great value. The cases in which I have prescribed it were of the usual run of every day practice, where a sedative or hypnotic was required—general restlessness, sleeplessness, neuralgia, catarrh, certain forms of skin-affections with great irritation, also rheumatism and gout. Many of my patients had some peculiarity of constitution which prevented the use of opiates of the usual type; and it is in this special class that I think urethan will prove of great value. It does not affect the nerve-centres of circulation or respiration, but spends itself on the cerebrum. Given in gout and rheumatism in full doses, alone or in combination, it has the great advantage over morphine of not interfering with the action of the bowels or kidneys.—*Brit. Med. Journal*, Feb. 20, 1886.



## In Memoriam.

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RICHARD ROBERT MADDEN,

F.R.C.S., Eng., M.R.I.A.

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THIS veteran member of the profession died at his residence, 3 Vernon-terrace, Booterstown, Co. Dublin, on Friday, February 5, at the ripe age of 87 years.

He was born in the midst of the stormy scenes of the rebellion of 1798, of which he afterwards became the chronicler. The youngest of twenty-one children, and the son of an eminent Dublin merchant, he entered at an early age upon the study of medicine. In 1829 he became a Member of the Royal College of Surgeons of England, and subsequently (in 1855) he was chosen to be a Fellow of that body. At the outset of his professional career MR. MADDEN married the youngest daughter of the late John Elmsley, Esq., of Berners-street, London, and of Surge Island, Jamaica. He entered the Civil Service in 1833, when he was appointed Special Magistrate in Jamaica. From that time the history of his useful life falls more within the domain of philanthropy and literature than within that of practical medicine, although happily for mankind all three great spheres of action dovetail into one another.

We cull the following account of his life from a memoir which appeared in the *Dublin University Magazine* for March, 1876 :—

“In one particular sphere of philanthropy, DR. MADDEN has worked with such fellow-labourers as Wilberforce, Buxton, and Clarkson—we mean the abolition of Slavery. The prelude to his vigorous exertions in this noble cause was his appointment, in 1833, as Special Magistrate in Jamaica. He no sooner set foot on the island than, with rigid and scrupulous straightforwardness, he spread a protecting arm around the poor negroes, whom he thenceforward did his best to guard from the miseries of hateful serfdom. Three years after this, DR. MADDEN was nominated Superintendent of Liberated Africans at Havannah, under the British Colonial Office, and in 1839, Acting Judge Advocate, in the Mixed Commission Court, under the Foreign Office. In these situations he found abundant scope for the exercise of his philanthropy, inasmuch as considerable discretionary authority was vested in him—if not actually

for slave emancipation, at least for diminishing the bitterness of thralldom. All such intrepid hostility towards tyranny made DR. MADDEN quite obnoxious to rich slavers and slave-owners; and on one occasion a mere accident averted the assassin's dagger.

"Such services were not forgotten, and in 1841 DR. MADDEN was chosen by Lord John Russell a Commissioner of Inquiry on the Western Coast of Africa. Here he laboured devotedly in the cause of humanity, and discovered that under the name of the 'Pawn System' an absolute state of slavery existed, and that, too, under the very eye of the authorities at the forts and posts established by the English Government for the protection of the negroes. During this appointment DR. MADDEN's surveillance included, among other territories in Africa, the West Coast, Gambia, and Cape Coast Castle.

"In 1847 DR. MADDEN was appointed to the Colonial Secretaryship of Western Australia, and three years afterwards he became Secretary to the Loan Fund Board, a position from which he retired a few years since.

"If DR. MADDEN had never written a line, his services in connection with the abolition of the slave trade would entitle him to public gratitude. If in the fearless discharge of his duty DR. MADDEN excited the enmity of the slave interest, he also won golden opinions from those who were really the negroes' friends. Not only abroad, but in his own country, men of the very highest eminence were foremost in recognising his signal abilities and services. It is seldom that a man succeeds in winning the unreserved approbation of such men as Lords Glenelg, Palmerston, Russell, Derby, and Normanby, as well as eliciting admiration of such members of his own profession as Gregory, Cooper, Brodie, Johnson, Crampton, Kirby, and O'Reilly. But perhaps the most valuable tribute came from the lips of Buxton and Clarkson, two of the finest champions ever furnished by humanity to the oppressed coloured race. With these men, as also with William Wilberforce, must be linked the name of our distinguished countryman.

"Notwithstanding the absorbing nature of his public duties, DR. MADDEN found time to cultivate his literary tastes, and acquire distinction as an author. In looking over his writings, besides admiring their quality and texture, one is amazed at the quantity—the more so considering his other avocations. He has written largely and excellently in the departments of politics, sociology, history, travels, and *belles lettres*. His works are so varied and numerous that we cannot refer to them in detail, but must content ourselves with briefly indicating some of the most important. No one who peruses DR. MADDEN's books can fail to appreciate their research, eloquence, and love of Fatherland, however much he may dissent from some of his opinions and conclusions. He traces the account of his country's vicissitudes with power and beauty, and leaves on record a great deal of valuable historic lore. The 'Lives and Times of the United Irishmen' make up a series of seven volumes, the publication of which

commenced in 1842 and terminated in 1846. With DR. MADDEN this work was evidently a 'labour of love.' He has undoubtedly displayed great ability, industry, and research in depicting the eventful and tragic career of the leading spirits who inspired the insurrections of '98 and 1803—men of whom it may be truly said, that if they loved their country, 'not wisely but too well,' their patriotism was, at least, unselfish and devoted, and so unlike the wretched travesty of our day, it had no mercenary tinge.

"Perhaps DR. MADDEN's best work is 'The Life and Martyrdom of Savonarola.' It affords evidence of high descriptive power, and is valuable, not only as a most interesting biography, but also as embracing the history that exercised a powerful influence on the future of the Church. DR. MADDEN writes with freedom and judgment, and his views are generally impartial and enlightened. He does full justice to the mission and genius of the great pioneer of ecclesiastical reform, and this is accomplished in a manner which leaves nothing to be desired. In fulfilling his task, he draws aside the curtain and paints with terrible accuracy, while reprimanding with emphasis the irregularity and vice into which ecclesiastics and their system has fallen since Savonarola's time. For his zeal in attempting to promote reformation, Savonarola paid the penalty of his life. That life, let foes say what they will, was one of purity and self-renunciation.

"Among DR. MADDEN's other principal works we may mention—'Travels in Turkey and Egypt,' 'The Mussulman,' 'The Infirmities of Genius' (a most agreeable and readable volume), 'Travels in the West Indies,' 'Egypt and Mahommed Ali, and Condition of his Slaves and Subjects,' 'Connection of the Kingdom of Ireland with the Crown of England,' 'The Island of Cuba, &c.,' 'Shrines and Sepulchres of the Old and New World,' 'The Memoirs of the Countess of Blessington,' 'Illusions and Fanaticisms of an Epidemic Character,' and 'The History of Irish Periodical Literature.' The last work constitutes, in its class, a most valuable addition to the national history of Ireland. Thus, while discharging with distinguished ability and fidelity the varied and onerous duties of a long official life, at home and abroad, DR. MADDEN found time to establish, by his voluminous writings, a deservedly high literary reputation."

To this tribute we have little to add. MR. MADDEN died as he had lived—an upright, honourable, serious-minded man, and his genial presence will long be missed by those who had the pleasure and the advantage of a personal acquaintance with him.

# THE DUBLIN JOURNAL

OF

## MEDICAL SCIENCE.

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APRIL 1, 1886.

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### PART I.

### ORIGINAL COMMUNICATIONS.

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ART. XV.—*Hyperpyrexia in Rheumatic Fever.*\* By W. LANGFORD SYMES, L.K.Q.C.P.I., &c., &c.; late Surgeon "Pacific" and "Orient" Royal Mail Services.

SOME interesting discussions having taken place on this subject from time to time, and on the best means of treating this most dangerous complication, I deem it my duty to record my own experience, inasmuch as every particle of reliable information is of the utmost importance, especially as the propriety of the application of iced water to the body in states of high fever is still a "*quæstio vexata*" amongst the profession, and universally deprecated by the general public.

The following case of rheumatic fever of a peculiar type came under my notice while on a voyage to South America, and it illustrates very well the liability to a sudden rise of temperature at any period of the disease, the height which it may attain, and the marked success of the much-dreaded treatment by the iced pack:—

CASE.—*Acute articular rheumatism; hyperpyrexia; temperature 108° Fahr.; delirium; acute endocarditis; use of iced pack; recovery.*—Mrs. B., aged twenty-seven, passenger from Liverpool to Valparaiso, was exposed for some hours to cold wind and driving rain on the 5th November, 1884, immediately before embarking. She had previously been in good health, and had no family history of any complaint. When off the Spanish coast, about five days later, she complained of severe pains in the ankles,

\* Read in the Medical Section of the Academy of Medicine in Ireland, Feb. 26, 1886. [For the discussion on this paper, see page 374.]

knees, and elbows, which joints were swollen and tender. Temp. 100°, pulse 140; tongue coated with brown fur; bowels confined; skin "clammy" with offensive perspiration, which had a distinctly acid reaction; headache and feeling of general malaise—in fact, all the ordinary symptoms of subacute articular rheumatism. The heart was perfectly sound; kidneys acting freely, the urine being full of lithates, but containing no albumen.

She was placed in hospital between blankets. The joints were wrapped up in cotton wadding and covered with oiled silk. She was put on "fever diet," and took the following mixture every three hours:—

R. Tinct. aconiti	-	℥ 2.
Nepenthe	-	℥ 15.
Sodii salicylatis	-	gr. 15.
Spt. chloroformi	-	℥ 15.
Aquæ ad	-	℥ss. Tertiis horis.

The bowels were opened by enemata when necessary, and in nine or ten days she was up and walking about the decks perfectly free from any pain or fever.

On the night of November 28th, having been convalescent for six or seven days previously, and able to get about, she got wet through while lying asleep in bed, by a sea coming in through the porthole; and next morning (29th) complained of the pains returning in the joints, and the old symptoms coming on again, the temperature being 99·4°, and the pulse 120. The joints were again wrapped in cotton wadding as before, and she was put on the same mixture. That evening the temperature rose to 102°, and the pulse to 140. The joints were very painful, so much so that she could not move in bed, the shoulders and hips now being involved. The heart was sound, acting regularly, but seemed at times a little excited, having a somewhat "thumping" action, for which I applied emplastr. belladonnæ. Her mind was clear, she had no headache, and was perspiring freely. On the following morning (Nov. 30th) her husband came to me at 8 a.m., saying his wife was "so much better," and wanted to know if she might get up, as she had "no pains whatever," and was "sitting up in bed." I found her, as he described, sitting up, looking very much excited and nervous, bathed in profuse perspiration, her eyes glistening, breathing rapidly, speaking very incoherently, and deliriously wanting to get out of bed and go on shore immediately. On taking her temperature the thermometer registered 106° in the axilla. Pulse 140, markedly intermittent, irregular in force and rhythm, and very much excited. She was alternately yawning, "going off" into short syncopal attacks, sighing deeply, and throwing her arms and legs about most recklessly. It was now quite evident what had occurred. On examining the heart a loud blowing, systolic sound was audible at the apex, extending round to the inferior angle of the scapula, and not

conducted into the cervical vessels, indicating mitral regurgitation. The heart was labouring violently for two or three pulsations at a time, and then became almost inaudible, dropping a beat at most uncertain intervals, and having no regularity whatever either in force or rhythm, no two beats seeming alike. I immediately gave her 20 grains of quinine, with 5 minims of tincture of digitalis and a little brandy, as an antipyretic with a view to subdue the high fever, and although this was partially retained on the stomach, the temperature, instead of, as I expected, having fallen, had risen in an hour to  $108^{\circ}$ . The delirium had increased. Her tongue was perfectly dry, and no improvement ensued either in the heart or pulse.

Seeing this effort to reduce the temperature had failed, I was at a loss to know what to do, fearing lest the shock of an iced bath might end fatally, being too severe and overpowering for the heart in such a precarious condition, and that she might die of syncope while in the bath. However, having no second opinion to consult on the matter, and knowing that the case must terminate in death if left alone, and that very soon—in spite of the remonstrances of her husband and numerous friends, I proceeded to apply the “iced pack” in the manner taught by Professor Finny (who has written a small pamphlet on the subject). Having divested her of all clothes, and placed a mackintosh underneath, towels wrung out of iced water were wrapped round her head, chest, arms, legs, and back. She was covered with blankets, and left thus for ten minutes. The first effect of this was most marked. She completely lost all her delirium, spoke rationally, and expressed her delight at feeling the icy cloths around her. She took some beef-tea and about a teaspoonful of brandy, and seemed much better in every way, and this during the first ten minutes. The towels when removed were burning hot, and the thermometer stood at  $104.4^{\circ}$  in the axilla, registering a fall of  $3.6^{\circ}$ . The towels were again applied as before, and remained for ten minutes, when the temperature had fallen to  $103.4^{\circ}$ . After the third and last application the temperature had fallen to  $97.8^{\circ}$ , thus making in all a sudden fall of  $10.2^{\circ}$  degrees in the space of 35 or 40 minutes. .

As will be seen from the chart, the temperature rose again that evening to  $100^{\circ}$ , and three days later to  $102^{\circ}$ . However, from this point she gradually began to improve; the pains diminished, and she was put on—

R. Tinct. ferri perchloridi	-	m 15.
Glycerini	-	3ss.
Spt. chloroformi	-	m 15.
Infus. calumbæ ad.	-	3ss. Ter in die.

She finally landed in Chili on December 15th free from any acute rheumatic symptoms other than a damaged mitral valve, which still remained to remind her unfortunately at some future date of the dangerous illness through which she passed.

# CHART OF TEMPERATURE, &c.

*Name, Mrs. B.; Age, 27; Disease, Acute Rheumatism; Result, Recovery.*

[illegible]

*Remarks.*—The foregoing case presents some interesting features, not that it is in any way unique, but an instance of remarkable recovery under very adverse circumstances. Commencing as an ordinary case of subacute rheumatism, and convalescence supervening on the usual treatment, its subsequent course was most unexpected, being marked by a sudden and severe relapse, happily not often met with. This teaches us that the very mildest cases of rheumatic fever are fraught with extreme danger, and that it is very difficult, if not altogether impossible, with our present knowledge, to foresee and predict the onset of hyperpyrexia, or the occurrence of complications during its course.

The importance of frequent and careful examination of the heart cannot be too stringently laid down, inasmuch as that organ is the one most frequently attacked by secondary rheumatic inflammation, and very serious mischief may be going on in the endo- or peri-cardium without the patient evincing any very decided symptoms. The exact changes in the patient's condition previous to the onset of hyperpyrexia chiefly took place during the night, so that I cannot record them accurately in their order of occurrence. However, I found the first symptoms of a change for the worse were—talking in her sleep, passing a very restless night, the gradual “improvement” in the pains, and being able to move some of the joints; and when I saw her in the morning delirium, acute endocarditis (and probably myocarditis), and hyperpyrexia were all present, and she was completely free from pain.

With regard to the hyperpyrexia I think nothing could have been more satisfactory than was its treatment in this case by the iced pack. Not only was the temperature brought down from  $108^{\circ}$  to  $97.8^{\circ}$  in less than an hour, but its soothing effect on the excited and delirious condition of the patient was most marked, the mind becoming clear the moment the ice was applied to the head. This sedative effect of an iced pack I have also repeatedly observed in cases of sunstroke with hyperpyrexia where the patient gets very excited, the temperature being as high as  $109^{\circ}$ , and on more than one occasion have I seen sleep follow its application.

It must not be expected that the temperature when brought down to normal by the iced pack or cold bath will remain at or below that standard for any length of time, for without any fresh cause or aggravation of the symptoms in any way it rose, in the foregoing case, to  $102^{\circ}$  three days afterwards, as if it were merely resuming its proper level in the chart, corresponding proportional<sup>1</sup>



with the other symptoms. Rarely, however, if ever, does it again reach its original height without death resulting.

That it is attended with some risk I think no one will contradict, and this chiefly from one of two causes—either sudden shock to the nervous system, caused by the enormous difference ( $76^{\circ}$ ) between the temperature of the skin at  $108^{\circ}$  and the water at  $32^{\circ}$ , evidenced in each case by the occurrence of severe rigors during its application, or else from sudden determination of blood to some organ, ending in an apoplexy of the part. Judging, however, from the case in question, it would seem that cardiac complications, even though they be very severe and acute, form no ground for its omission, so that, unless these theoretical objections are frequently realised in practice, they ought to be disregarded in such a crisis, where other means have failed and time lost may mean a life sacrificed. It seems a pity that a treatment so beneficial in extreme cases of hyperpyrexia should need defence, and yet the aversion which the public entertain towards it is so strong, and so bigoted is their prejudice against it, that they regard it in many cases simply as an unwarranted experiment. This being the case, it explains the fact that the experience of the pack or cold bath is almost exclusively confined to hospital practice.

Touching the interesting and important subject of the relation of complications in rheumatic fever, very little is known at present. It has been stated that the perspiration ceases before the rise in temperature is observed, but this cannot be put down as a clinical fact, inasmuch as perspiration is sometimes profuse during the entire attack. In some cases that I have observed the solids of the urine have suddenly ceased to be eliminated in sufficient quantity, the urine being pale and specific gravity being diminished before hyperpyrexia sets in; but whether this is a common antecedent to complications I cannot state. This, as well as many other difficulties in connection with this affection, will have to remain for its solution until the combined experiences of practical physicians and the scientific researches of learned physiologists unite to enlighten us in the pathology, prognosis, and treatment of rheumatic fever, and reduce to a minimum the mortality from this most treacherous disease.

ART. XVI.—*Axis-Traction in Instrumental Delivery, with Description of a New and Simple Axis-Traction Forceps.* By WILLIAM C. NEVILLE, M.A., M.D., and Master in Obstetric Science, Univ. Dubl.; M.K.Q.C.P.I.; Fellow, Honorary Secretary of the Obstetrical Section, and Member of the General Council, Academy of Medicine in Ireland; Physician to the Pitt-street Institution for Diseases of Women and Children, Dublin, &c.

[Concluded from page 109.]

ON first considering the mechanism of axis-traction forceps I was much struck by their general uniformity. So far as I have been able to ascertain, all who have hitherto modified and improved Tarnier's forceps have been content to retain his device of making the traction force pass through steel rods united to the blades by pivot-joints immediately below the fenestræ. Yet it is easy to show that many disadvantages are inseparable from the presence of such traction-rods. They add to the complexity and increase the difficulty of introducing the instrument; to some extent they interfere with the pressing backwards of the shanks against the perinæum, which is essential if the blades are to be properly applied to the head situated at or above the brim of the pelvis, special care being needed in such cases, so as to avoid the risk of injuring the anterior edge of the perinæum; and they introduce the main difficulty in such forceps—that, viz., of attaching the rods to the traction-bar by some device which shall be at once secure, simple, and easy to manipulate. It is quite true that such drawbacks as these, even when considered collectively, are of but trifling import when weighed against the decided advantages of these forceps, but they indicated an effort at least to devise some method by which the same good results might be achieved without them.

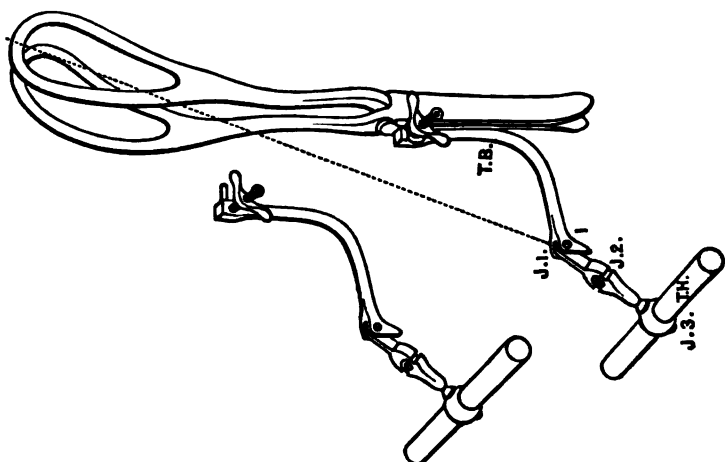
The question I proposed to myself was, then—Could an axis-traction forceps, with all the advantages of Tarnier's, be constructed without using the device of traction-rods? Theoretically, I thought I could see my way clearly enough to the solution of this problem, though I found the practical mechanism very difficult to work out successfully. I am quite sure that any intelligent practical mechanic—if I could have met such—would have saved a good deal of my time, and settled the question much more quickly and neatly than I have been able to do.

Messrs. Fannin & Co., of Grafton-street—to whom I am much indebted for the trouble they have taken in carrying out my plans—

have constructed an axis-traction forceps, without traction-rods, upon the following lines:—

The general plan of the instrument will be understood by a reference to Fig. 4, which shows the forceps with its traction apparatus affixed. The forceps used may be a Barnes', Simpson's, or any other long double-curved forceps, according to individual fancy. I prefer the first-named because of its greater length, which allows the head to be easily seized, even when situated above the pelvic brim. A short way below the lock a re-curved steel bar (T B) is rigidly fastened to the handles by means of a special device presently to be explained. This bar curves backwards sufficiently far to reach the imaginary continuation of the axis of the blades, marked by the dotted line in the woodcut. At the point of intersection the bar turns downwards in the line of the continuation of this axis, and is immediately finished off as an indicator (I), which serves to show the proper axis of traction. A pivot-joint (J 1), admitting of free motion in the horizontal plane, unites this traction-rod to another shorter one, which terminates in a screw-joint (J 2), so formed as to admit of motion in a plane perpendicular to that allowed of by the previous joint. From this joint a short bar continues the apparatus onwards to the transverse traction-handle (T H), with which it is connected, exactly as in Tarnier's forceps, by a joint allowing of a rotatory motion (J 3).

Fig. 4.



When, with this forceps, traction is made through T H in the axis of the forceps—i.e., in the axis of the pelvic canal (approximately), as shown by the indicator (I), the force acts directly from J 1, exactly as if the traction were made from the extreme end of the handles of such a forceps as Aveling's. Mechanically, everything depends on the practical rigidity of the junction between the traction-bar and the forceps handles. So far as the indicator this forceps is practically the same as Hubert's second forceps, previously described, and from that on we have merely a device, modified from Tarnier's, for allowing mobility to the head while traction is being applied. The three joints admit of complete mobility, and the indicator shows the direction in which the tractions are to be made. It must be clear, however, that in practice such an indicator is scarcely needed, the proper axis of traction being otherwise sufficiently apparent. In the junction between the traction apparatus and the forceps handles I have adopted the device suggested by the employé of Messrs. Fannin, who made the first rough model for me. The traction bar terminates in a T-shaped extremity, one end of the T being finished off as a screw, while a rounded stout pin projects downwards at right angles to the other end (Fig. 5). The forceps having been applied as usual, the traction apparatus is then affixed to it as follows:—The rounded pin is first fitted into a hole specially bored for it in one of the handles (the left), and the screw end of the T is then rotated into position in a slot fixed to the other handle. The blades are then fixed by the nut attached to the screw. This device gives perfect rigidity when the forceps is so applied as to lock well with parallel handles, but I still think that some simpler plan might be devised.

For this forceps I claim that—

1. It gives all the advantages of Tarnier's over the ordinary double-curved forceps.
2. It is a much simpler instrument, and more easy to apply than Tarnier's, or any of its modifications.
3. It avoids the disadvantages of traction-rods, and the traction apparatus is entirely outside the vagina in sight.
4. The traction-apparatus described can be easily fitted to any ordinary double-curved forceps.
5. The forceps can be used with or without the traction apparatus.

I am greatly indebted to Dr. Macan, Master of the Rotunda Hospital, for having practically tested the first model of this forceps

in that institution, and am gratified to hear from him that it worked well. To another friend, Professor FitzGerald, F.T.C.D., I am also indebted for the advice he kindly gave me on the theoretical side of the mechanics involved; and I am strengthened by the opinion of such an authority that the forceps described has essentially the same mechanical power as Tarnier's.

I believe that, for the reasons given, this axis-traction forceps is superior to any with which I am acquainted; but, though attaining its results in a different and simpler way than that which, following Tarnier, has hitherto been constantly adopted, the objects aimed at are precisely the same. Whatever modifications have hitherto or may hereafter be made, with a view to perfecting the mechanism of these instruments, the credit must always remain with Tarnier of having been the first to formulate the essentials of a good forceps with scientific exactness, as well as of having shown in no crude form how the theoretical indications might be practically fulfilled. I do not doubt that in doing so much he has made an epoch in the history of the forceps, or that in one form or another the principles for which he contended—axis-traction and liberty of movement for the head during traction—will gradually be universally adopted. I hope that the instrument I have described may prove at least to be a suggestive simplification of those previously designed to admit of the practical application of these principles.

ART. XVII.—*A Case of Diabetes Mellitus, with Remarks.* By J. STRAHAN, M.D., Belfast; Fothergillian Gold Medallist, Med. Soc., London.

MANY of our best authorities (Sir W. Roberts, Beale) consider that diabetes in the aged, or in the obese, is attended with hardly any danger to life. One writer of high standing says he does not believe that in the aged life is even shortened by the affection.

The rule, in cases occurring in the young and the spare middle-aged, is that life is never safe with persistent glycosuria, as even where a case seems to be doing well, sudden failure may occur, and various nerve lesions or coma cause death. My case would go to show that the rule had better be extended to all ages and conditions. The patient in this case was a very large, tall woman, enormously stout, weighing for many years 20 stone (280 lbs.), and was over seventy-one years before serious symptoms developed.

Diabetes commencing at so late an age is very uncommon ;

hence, perhaps, the failure to note its dangers. Especially is this the case in hospital practice, whence most of our statistics are drawn. Dr. Pavy, in the statistics of his private practice, only met with 35 cases commencing at 70 or over, out of 1,360—or 2·56 per cent. He says that his hospital practice would show few cases commencing at over 40 years (*Brit. Med. Jour.*, Dec. 5th, 1885).

In the following case I had the great advantage of consultation and co-operation with Professor Dill, Queen's College, Belfast, our worthy city coroner, but he is not in any way responsible for the statements or opinions here expressed:—

CASE.—Mrs. M. had been a widow for thirty years; had lived well, but was almost an abstainer from alcohol; had a very comfortable, and not very anxious, life as mistress of her brother's house, a well-to-do medical man. She had always enjoyed good health, until shortly before her brother's death, early in 1882, when she had a severe carbuncle on the nape of the neck, followed by peculiar patches of deep-seated cellulitis on the toes and soles of the feet, which quickly sloughed, leaving cavities about the shape and size of a hen's egg cut in half. For eight months or so, these spots kept appearing in groups of two or three, at intervals of weeks or months, and for these she first came under my care. They healed very slowly, no matter what local applications were made, and there was a marked absence of foul smell even when the slough was separating (without any antiseptic). This absence of bad smell has been noted in cases of gangrene of the lung occurring in diabetes. She sometimes noticed a large excess of urine for a time, but only intermittingly. Her general health was excellent, with the exception of slight constipation. The recurrence of the sloughs gradually ceased while taking opium and arsenic. I suspected diabetes, but, depending on authority, and considering her good health and vast weight, did not think it essential to alarm and distress her by making an analysis of the urine. She was at times very irritable, sad, and hypochondriacal, but at the same time apathetic. From the confidence existing between her and her attendant (a servant of twenty-eight years' standing), I could not have obtained a specimen of urine without her knowledge, so I considered it best to content myself with directing her to live chiefly on meat, fat, and vegetables, and to avoid sugar and all starchy articles. According to our authorities, this is the best and quite sufficient treatment under the circumstances. While suffering from the local gangrenes she contracted very sedentary habits, which were continued after her recovery, so that she hardly ever left the house again, until the occasion which upset the balance of her system and was the exciting cause of her death. Still, I could not ascertain that she had any of that feeling of weakness

and languor which is so marked a feature in most cases of diabetes. She had a good, but not voracious, appetite; did not notice any particular thirst; tongue inclined to be dry, but not extra red. As her brother (a man of 18 stone) had died, as was said, of heart disease, she had her heart examined. The action was weak, the impulse faint, the first sound short and valvular, no murmur—only deep percussion gave any result, owing to the thick chest wall, and that indicated slight enlargement—in fact, one could hardly say whether the heart was fatty or only loaded with fat. But as fatty heart is usually found in diabetes in the aged, and from the mode of death, the probability is that both conditions existed.

On August 1st, 1885, I was called to her. She stated that on going to church that morning her limbs had suddenly given way below her, without any loss of consciousness or even mental confusion (as would occur in cerebral hæmorrhage or softening), and without any pain or feeling of weight in the back (as would occur in spinal hæmorrhage from unwonted exertion). She reached home with difficulty, leaning on her nephew. In the evening there was almost complete paraplegia, motility being quite, and sensibility almost, abolished. Reflex movement to tickling, &c., abolished; skin very cold; no œdema of legs, although Dr. Garrod says such is constant in diabetes. There was now a great deal of languor and feeling of extreme tiredness; tongue red and dry, but not beefy; breath sweetish; complete anorexia; bowels confined. The temperature was 98° F., although the weather was very hot; room at 70° F. The heart's action very feeble; pulse 70, small, short, and soft. The paralysis affected the bladder, so that the patient had no power to empty it, and the catheter had to be used. There was not the slightest symptom of a cerebral origin of the paralysis; pupils normal, and reacting to light; mind perfectly clear; speech perfect; swallowing perfect; no deviation of tongue or features; no symptom, in fact, of hemiplegia. The arms were not at this time paralysed. No pain in back, limbs, or elsewhere; no spasms; no fever. There was a tendency to syncope, from faint action of the heart; and, immediately after I left, she fainted, even in the horizontal posture, so that I had to return, and with difficulty restored animation. She was ordered very hot milk, with half-ounce doses of spirits frequently; hot applications and plenty of blankets; also, a large hot enema, with carbonate of ammonium and belladonna, to recover her from the state of collapse.

The urine passed was about 100 ozs. in 24 hours, pale yellowish green in colour, acid, with a smell like stale vinegar; sp. gr. 1050; no albumen; urea a good deal increased. Fehling's test showed plenty of sugar. On employing Sir W. Roberts' quantitative fermentation test, the specific gravity was reduced to 1010, thus losing 40 degrees of density; so that the urine contained about 40 grains of sugar per ounce; she was thus excreting 9 ounces, 162·5 grains daily.

For four days the case remained much the same, but the symptoms gradually extended and deepened. The paralysis extended to the arms—the left first. At the end of the fourth day she could move no part of the body except the head and the right arm feebly. Deglutition now failed rapidly, and soon even the head could not be turned or lifted. Still the features were undeviating and the speech perfect, but faint; mind clear.

On the fifth day prostration was complete. She was quite incapable of moving; sensibility and motility abolished; pulse frequent and thready; tongue dry and dark; the temperature  $90^{\circ}$  F.; skin corpse-like in coldness. There was violent dyspnoea, characterised by deep inspiration and expiration, rapid and regular, just as if out of breath from exertion, without intermittence or hissing, as in uræmia; urine plentiful, had still to be drawn off; quantity of sugar had remained pretty constant. It seemed as if diabetic coma were coming on, when death suddenly occurred from cardiac failure, in presence of Professor Dill and myself. Consciousness remained perfect till the end.

*Remarks.*—There was no *post mortem*. The case seems an illustration of the extensive derangements of tissue changes generally, which lead to general deterioration, to many local diseases, and often to death. The muscular tissue of the heart is relaxed and fatty in diabetes in old age, and the small and medium-sized vessels atheromatous, the atheroma being more decided in the cerebral vessels at the base than elsewhere. The immediate cause of death in this case seemed to be failure of the heart from glycaemic degeneration of its fibres, combined with nervous failure; while the whole train of serious symptoms seemed to be set up by the, for her, unusual exertion of walking half a mile to church. As diabetes is so strongly hereditary and so distinctly runs in families, it seems to me that the brother of this patient was also diabetic, and seemed to die in much the same way, by cardiac failure—in fact, of diabetic heart disease.

The nervous phenomena of diabetes, including true diabetic coma, seem to depend on different causes in different cases. None of the common theories will explain all cases. Sanders and Hamilton have suggested fat emboli of the lungs and brain as explaining the dyspnoea and collapse in cases such as that described. But the theory fails in more points of view than one. First, the milky state of the blood is not constant, and Dr. Pavy believes it to be purely a physiological condition, due to the quantity and nature of the food consumed by diabetics, the quantity being usu<sup>all</sup>



large and the quality albuminous and fatty, by prescription. Again, the blood is frequently normal in its appearance; sometimes thick and dark-coloured. Secondly, fat emboli have been found in many conditions without coma, or, indeed, any symptom. In fractures of bones and even in contusions they are usually present, but produce no symptoms whatever. The uræmic theory of Wunderlich, Busch, and Griesmeyer, may be dismissed at once—the great difference between diabetic nervous phenomena and those of uræmia, both in a clinical and pathological view, being strongly against the theory. The evidence in favour of Kussmaul's theory of acetonæmia is still inconclusive; but, no doubt, the presence in the blood of some toxic agent, either acetone, aceto-acetic acid, or some other undetected product of the vinous fermentation, will account for many cases of diabetic nerve phenomena.

The "dehydration of the tissues" theory of Bouchard and Hilton Fagge fails from the fact that the blood in diabetic coma does not constantly present the thick and dark-coloured, or "tarry" appearance, although it is, no doubt, present in many cases.

Dehydration of the tissues must be present to some extent in every case of glycæmia, and is, no doubt, one element in the deterioration of nutrition and consequent degeneration of all the tissues.

But, associated with exosmosis of water from the tissues must be endosmosis of sugar into the tissues, and this is probably an equally efficient element in the depravation of nutrition. However, this endosmotic theory, when applied to diabetic cataract, shows the difficulties in the way of its general application. This theory was suggested by the production of cataract in frogs by immersion in a saccharine solution, or by injecting syrup into the cellular tissue. The objections to it are the temporary nature of the saccharine cataract in the frog; the occasional occurrence of diabetic cataract in one eye only in man; the fact that diabetic cataract usually occurs only in cases of two years' or more duration; that it is present only in from 5 to 10 per cent. of all cases (Beale); and that it does not occur at all in some of the worst cases, while it is well marked in some very mild cases. Again, the usually slow development of the cataract would seem to show that it is a mere degeneration, determined in some unknown way. Still further, intravenous saline injections have been tried in diabetic coma by Dr. Hilton Fagge with only slight temporary improvement. Dr. Barth (*L'Union Méd.*, Aug. 7th, 1883) and Dr. Dreyfous consider that the glycæmia alone is a sufficient explanation of nervous

phenomena in diabetes; by depraving tissue nutrition generally, life is placed in danger, and any extra fatigue or other cause of nervous derangement causes sudden failure of the powers of life. For the same reason, diabetics are much more liable than others to inflammations of an asthenic character in all the organs, so that any slight exposure may be the cause of death. Thus, pneumonia in the diabetic is commonly fatal. Tubercular disease of the lungs occurs in nearly half the cases lasting more than one or two years.

Dr. Pavy holds the same views. He thinks coma results from "deprivation of power in certain nerve centres." He does not believe at all in acetonæmia, or any other direct poison in the blood.

Various motor disorders, hemiplegia, and paraplegia, have been known to occur in diabetes since the days of John Rollo (1796), who first adopted the animal diet system in diabetes. Such affections often depend on cerebral hæmorrhage, softening, or cerebro-spinal meningitis; but in the case related there was no indication of any local causative lesion whatever within the skull or spinal canal. The paralysis seemed to be a nervous phenomenon belonging to the same category as true diabetic coma—*i.e.*, coma without cerebral hæmorrhage or other coarse lesion.

Nervous phenomena other than coma are probably common enough, but have been overlooked in the same way as coma has been until within a few years. And yet probably more than half of all the fatal cases of diabetes die of coma. For instance, at the London Hospital, in a series of 37 fatal cases recorded by Dr. Stephen Mackenzie, 19 died of coma; and Dr. Frederick Taylor recorded at Guy's Hospital 53 fatal cases—33 of these died of coma.

Frerichs, in his work on Diabetes, gives a similar result from an analysis of a large number of cases. Out of 250 fatal cases of his own, 18 died from exhaustion, 34 from phthisis, 7 from pneumonia (4 of these had gangrene of the lungs), 8 from nephritis, 7 from carbuncle, and 9 from other complications (cancer in 6 cases). In the whole of the remaining 167 cases symptoms of cerebral paralysis appeared—in 10 due to hæmorrhage, in 2 to softening, in 3 to cerebro-spinal meningitis. In all the rest—*viz.*, 152 cases—death occurred from true diabetic coma—*i.e.*, coma without local, cerebral, or spinal lesion.

ART. XVIII.—*Experimental Researches upon Tuberculosis and Scrofula.* By EDWIN WOOTON.

[Concluded from page 247.]

Class 4. *Stimulants and Tonics.*—Energy-producing drugs include all those whose administration calls forth increased activity on the part either of the whole body or of particular organs. This energy may be produced by :—

- |                                  |   |   |   |                                 |
|----------------------------------|---|---|---|---------------------------------|
| Food for nutrition,              | - | - | - | Medicinal foods.                |
| Trophic influence promoters,     | - | - | - | True tonics.                    |
| Stimulators of activity,         | - | - | - | Stimulants, or indirect tonics. |
| Conservators of energy,          | - | - | - | Conservative tonics.            |
| Regulators of organic functions, | - | - | - | Regulating tonics.              |

There are drugs which act in some one only of these various ways; others exert their influence through two, or even the whole four of the methods indicated. In a very extended sense all such drugs are tonics; but the use of the term is inadvisable, as its inclusive character is apt to occasion misapprehension. Moreover, every drug, the taking of which results in improved health, is scientifically considered a tonic; but such general application of the term would result in its loss of all distinctive meaning, rendering it merely synonymous with medicine or physic.

The classification I have given, while aiming at scientific accuracy, will, I believe, be found suited for the exigencies of every-day practice.

Of medicinal foods I have already spoken.

The principal drugs that come under the heading of true tonics are—phosphorus, phosphoric acid, iron, silver, and zinc. All these, properly used, exert a general trophic influence; they restore the processes of nutrition in debilitated conditions, and the beneficial results they produce remain when their administration has been discontinued.

Phosphorus acts as a tonic to the whole body, by means of the raised trophic influence of the nervous system. But this latter is due not merely to increase of tone in the nervous centres, but also to their stimulation.

Stimulants comprise not only drugs acting on the whole system, but those which call forth temporarily the depressed power of certain organs in particular without first restoring their nutrition. Thus ether is a general stimulant, while taraxacum, nux vomica,

and gentian have respectively a hepatic, nervine, and gastric action. By the activity these local stimulants call forth they may lead secondarily to better nutrition of the whole body, including the organ primarily concerned.

Conservative tonics are of two kinds—those which act by preventing tissue waste, and those which render effete material capable of beneficial re-introduction into the system. Hence the second class of conservative tonics become medicinal foods.

Alcohol, tea, coffee, tobacco, and quinine lessen tissue waste by oxidation. The first three are in addition stimulants, increasing tissue change; while alcohol itself is in very small quantity a brain food. So that in estimating the value in the animal economy of these drugs, we must carefully consider the relative importance of—(1) the tissue saved, (2) the tissue destroyed, and, in the case of alcohol, the demand for this compound in the nervous system. These points have to be determined in each case by the peculiar physiological conditions concerned, and which depend on the age, occupation, and health of the individual.

Again, bromine acts as a conservative tonic of the first class by diminishing brain wear. As examples of the second class of conservative tonics may be mentioned the mineral acids.

Regulating tonics are drugs which do not act primarily by improving cellular structure, but which by their influence—stimulant or depressant—on the functions of localised whole organs, bring these to the normal, and hence improve secondarily the body's tone, and ultimately, through regulated action, the nutrition of the organ itself.

Conservation of energy by inhibition of oxidation, although temporarily conducive to a general sense of well-being, is ultimately injurious by preventing the excretion of effete material. Hence, as tonics, quinine, and other drugs of the same class are of true service only when the system is so enfeebled that, cellular recuperation being performed with difficulty, it becomes an object to keep the tissues as much *in statu quo* as possible. All the experiments I have performed show that quinine used under other conditions is absolutely hurtful.

Now in laboratory practice with dogs, cats, guinea-pigs, and rabbits, there is not a single stage of tuberculosis, under any of its forms, wherein quinine exerts any beneficial influence. On the contrary, its action is injurious throughout the disease, and in some stages hastens the progress of the malady to a fatal end. It is in

pulmonary tuberculosis that the deleterious influence of the drug is most apparent. The very presence of tubercle in the lung diminishes the oxidation of the blood. And this lessening becomes more and more marked as the lesions spread. If quinine be given when this form of tuberculosis obtains, and more especially in the advanced stages, the blood may be so imperfectly oxidised as to occasion death from dyspnoea. From the cases I have had under experiment, I find that of the two classes—the one treated with quinine, the other left wholly to the operation of the processes at work—those treated with quinine arrived the sooner at a fatal termination.

Alcohol would be as objectionable as quinine; but besides its conservative action by lessening oxidation, it is as noted a food and a stimulant. Moreover, if given freely, it runs in the blood for some time unaltered, and thus has an antiseptic action at the seat of any lesion.

As the object of the physician in dealing with tuberculosis is to call forth or stimulate the energy of the living bodily machine that it may incorporate into its substance the medicinal and dietetic foods given, and so gain real strength, as well as to endeavour to attack more directly the abnormal conditions of local regions—alcohol being a nerve food and stimulant, is valuable. Certainly the beneficial effects resulting from its use more than counter-balance the mischief which is the inevitable sequence of lessened oxidation. Moreover, we can give oxygen medicinally as a gas, or in combination as a drug, and the result is to altogether negative the inhibitory effect of the alcohol. I have employed the drug in the laboratory throughout tuberculosis in its various stages and forms, and I have found it of value chiefly in two ways—first, in commencing medical treatment, by its stimulant action on the nervous system, and hence on the absorbents; and next, as an antiseptic in the advanced stage of pulmonary disease. When the system has been stimulated and medicinal foods are being readily absorbed and incorporated, I have not found any marked advantage to result from its employment. Under its influence the animal and saline matters in the urine were invariably increased, due probably to stimulated expenditure of force and destruction of tissue; and although larger quantities of the phosphates and other such foods were absorbed than without the use of the drug, there were no signs of corresponding physiological benefit. Increased supply and increased expenditure left no apparent difference

in the balance. In the human subject, however, the value of alcohol is partly in its subjective effect; and therefore when the sense of health is increased by its use it may well be employed, even although we know that chemical or structural benefits are not to be looked for.

*Free Phosphorus*.—As a stimulant I have found this drug of service when commencing treatment, and in dealing with phthisis purposely neglected until the advanced stages were reached. Throughout the latter it can be given with advantage; but its constant use from the commencement is not attended with desirable results. In the first place, it does not supply the material for the energy it discharges. Then, even in small doses, long taken it causes fatty infiltration of the liver. And lastly, it is to a limited extent a cumulative medicine apt to give rise to symptoms of acute poisoning.

*Phosphorus* in itself is not poisonous; its fatal powers, like its therapeutic, are due to the compounds it so rapidly forms.

*Phosphoric Acid* and the other mineral acids may be used in combination as conservative tonics throughout the disease.

*Nux vomica* and other local stimulants are valuable at the commencement of treatment. I have, however, never found benefit to result from their continued employment.

**Class 5. Antiseptics and Septicides.**—The processes involved in protecting a tissue against the action of living forms, and in the destruction of these forms, are essentially distinct in their character. This has only very lately been recognised; and whereas formerly all drugs inimical to the development in the body of septics were without discrimination styled *antiseptics*, pathologists now very generally distinguish them into two classes: those protecting the tissues—to which the old name is applied; and those destroying the forms of life—disinfectants.

I accept the two terms as scientific, but each is capable, I submit, of accurate subdivision as follows:—

Tissue protectives—Antiseptics,	-	-	{ Common.
			{ Specific.
Septicides or disinfectants,	-	-	{ Common.
			{ Specific.

The blood is naturally antiseptic.

From an extensive series of experiments, the results of which I purpose giving to the world under the title of “Specific and Common Septicæ,” I have found that living forms never multiply

in the vascular system; that when they exist therein their seat of propagation is either a lesioned tissue, or the effete and excreted matters to be found in the respiratory and digestive tracts, as well as on the body surface.

Drugs having a definite action on one or more of the specific forms invariably act protectively or destructively, as the case may be, against the common forms. But the converse is not the case.

The following table is intended to show the method of application I have found most efficacious in the case of the chief septicides and antiseptics. One or two of these have a lethal action on certain specific febrile "forms":—

		Antiseptics : for internal employment { Glycerine. Alcohol.	
Septicides or Disinfectants.	For internal employment	{ Peroxide of Hydrogen. Permanganate of Potassium. Carbolic Acid. Creasote.	
	For inhalation	{ Peroxide of Hydrogen. Permanganate of Potassium. Sulphurous Acid.	
		{ Corrosive Sublimate } Only when septic processes have Amyl Alcohol } been subdued. To prevent ger- mination.	
	For inunction	{ The employment of septicides in this manner is alto- gether needless.	

*Exercise, Climate, Massage, &c.*, are therapeutic agents obviously beyond the sphere of the experimentalist's range of research.

As to climate, the thermal and atmospheric conditions employed by the worker must, either in their production or attending circumstances, be more or less artificial and unnatural; and results obtained are not at all reliable. These matters are for the decision of the clinical observer.

*Electricity.*—My own experiments with this agent so closely agree in their results with the observations recorded by medical scientists, whose studies have been made at the bedside, that I have little to add to our stock of facts concerning it. The continuous current is of value throughout the disease. It improves *all* the physiological functions, and must be held to occupy one of the highest places in the list of agents. The interrupted current I have found of service chiefly in the pulmonary form of tuberculosis, and then to bring about removal of impacted matters. Its application should be preceded by warm inhalations—medicated or not as the severity of the case may indicate—or by laxative expectorant

drugs. By this means the bronchi become relaxed, air passes below the impacted mass, and the subsequent contraction of the bronchial walls results in the expulsion of the impaction.

*Scrofula*.—The pathology of scrofula has been already described. We have seen that the processes involved in this condition and in tuberculosis are identical, but that the two diseases commence at different points in the circle of causation. The therapeutic agents which control tuberculosis exert an equal influence on scrofula. All that has to be done in commencing treatment is to diagnose accurately the conditions obtaining, and to apply to these the already proved remedies. Separate details concerning the therapeutics of scrofula are not therefore required, and would necessitate a repetition of facts already stated.

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#### THE TOPICAL USES OF LACTIC ACID.

PROFESSOR R. V. MOSETIG-MOORHOF, of Vienna, has become an ardent advocate of the use of lactic acid in the treatment of certain morbid growths and ulcerations. His earliest results were obtained with lupus, but he has since used the acid with good success in epithelioma, superficial caries, papillomata, and other new-growths. At a recent meeting of the Royal Society of Physicians of Vienna, Moorhof presented his views again (*Wiener medic. Wochensch.*, November 28, 1885), and also showed two patients with extensive epithelioma of the face who had been greatly relieved, if not cured, by the acid. The especial claim made for lactic acid is that it picks out and destroys morbid tissue, while it has very little effect on the surrounding sound structures. It can be applied to the healthy skin for twenty-four hours without producing more than a redness. Lactic acid can take the place of the sharp spoon, and is, in most cases, a more searching and thorough agent. For superficial application to ulcerating or carious surfaces the pure concentrated acid is used. Absorbent cotton soaked in the caustic may be laid upon the affected part and covered with rubber paper. The surrounding healthy tissue can be protected by covering it with the rubber or collodion. The application is left on from six to twelve or twenty-four hours. It is often so painful that the patients cannot tolerate it for the longer periods. After removing and washing, the part is allowed to rest for a day or so, and then the application is renewed. Moorhof showed patients upon whom he had made twenty and twenty-six applications. Parenchymatous injections into the pathogenic foci may also be given. In this case watery solutions of the strength of fifty per cent. to seventy per cent. are used, and from six to fifteen minims injected at a time. Several of these injections may be made at one sitting.—*The Med. Record*, Jan. 23.



## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

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*Diseases of the Tongue.* By HENRY T. BUTLIN, F.R.C.S.; Assistant Surgeon and Demonstrator of Practical Surgery and Diseases of the Larynx, St. Bartholomew's Hospital. London: Cassell & Co., Limited. 1885. Pp. 451.

THE book before us forms one of Cassell & Co.'s clinical manuals on special subjects, and fully testifies to the excellence of the series, should the succeeding volumes deal with their special subjects in as thorough and clear a manner as the present manual. Were proof needed that monographs on different subjects were required in a form readily accessible and easily consulted, it is furnished by Mr. Butlin's manual on Diseases of the Tongue. Recognising the difficulty frequently experienced by those who wish to look up a subject in finding in any work on general surgery the points which they specially want to investigate, Mr. Butlin tells us he has endeavoured, as far as possible, to follow the *encyclopædic* method. He has been obliged, however, to modify this to a certain extent in the arrangement of the chapters, as a strict adherence to the *encyclopædic* method would have necessitated a much larger number of small chapters, and the massing together of several of the larger chapters into one of very great length. The index and the synopsis of the contents will, he trusts, supply any defect in this respect.

Reference to the different subjects treated of is much simplified by printing in thick black type the headings of the various paragraphs, and the commencement of a fresh part of the subject, such as the diagnosis or the treatment, is in many cases indicated by printing a prominent word in italics.

The first subject dealt with is the Accidents to the Tongue—Burns and Scalds, Stings and Bites, Wounds and Foreign Bodies in the Tongue.

At p. 11 the author discusses the methods of dealing with hæmorrhage consequent on a wound, and impresses the importance

of arresting the hæmorrhage *thoroughly* as soon after the accident as possible:—

“If the wound is far forward there is usually no difficulty in doing so; but if the wound is far back, a difficult and serious operation may be required. In such a case let the bleeding be temporarily arrested by the pressure of the finger on the wound, or a pad of lint between the finger and the wound, until the measures for the permanent arrest have been considered and arranged. Then place the patient in a good light, administer chloroform if possible, and open the mouth thoroughly with a strong gag, draw out the tongue by two stout threads passed through its tip, one on either side, and carefully examine the wound. . . . If, when the wound is thus exposed, a bleeding vessel can be seen, it will of course be ligatured; but if blood wells up from a deep and perhaps punctured wound, the wound should, without hesitation, be enlarged until the vessel is in view. If on examination it appears certain that the hæmorrhage is not arterial, but is the result of general oozing or of wounds of veins, the bringing together of the edges of the wound by deep sutures, after the clots and other matters have been removed, will suffice to arrest the bleeding. . . . Ligature of the lingual artery in the neck is probably never needful in cases of primary hæmorrhage from the tongue. . . . Severe and repeated secondary hæmorrhage may necessitate ligature of the lingual artery.”

We have quoted this passage because we consider that the advice given is based on sound surgical principles and as indicating the lines of practice which should be adopted in such emergencies before resorting to more heroic measures.

At p. 18 a curious instance is given of congenital absence of the tongue, which shows what may be expected as regards the power of speech when the organ has been totally extirpated:—

“At the age of fifteen years the girl described by Jussieu had, in place of a tongue, a small elevation in the middle of the floor of the mouth, about three or four lines in height. It was to a certain extent movable, the muscles at the base of the tongue being fairly developed, and was evidently very useful in speaking and swallowing. Speech was very little affected by the absence of the tongue; it was, indeed, so clear that no one would have suspected that so serious a defect existed. . . . Mastication and the swallowing of solid food were the only acts which were really difficult to this patient.”

At p. 23 an interesting paragraph on Tongue Swallowing will be found. Two cases of this rare accident were described by Petit, in 1742, occurring in children, and a third case where the child was

reared with difficulty. The cause is attributed to a congenitally elongated frænum, or to division of the frænum shortly after birth. In the two fatal cases the children died suffocated, from drawing the tongue back into the pharynx during the act of swallowing. On *post-mortem* examination "he found the throat completely filled by the tongue, the tip of which was turned back over the dorsum and fixed, like a wedge, in the upper part of the gullet. The obstruction to the larynx was so complete that no air could possibly have passed into the lungs. A case recorded by Fairbairn differed in some respects from those by Petit. The child had a cleft palate, and whenever more than a very small quantity of liquid was dropped into its mouth it was attacked by cough and threatened with suffocation. When it was two days old it died, apparently from suffocation. At the autopsy the tongue was found to be short and thick, with a very defective frænum. The tip was not retroverted, but the whole tongue lay so far back that only the tip was visible at the back of the mouth. The dorsum was applied against the back wall of the pharynx, and the base pressed down on the epiglottis and arytenoid cartilages in such a manner that the entrance to the larynx was completely lost. Fairbairn said that he had had another somewhat similar case, but the infant had been reared."

As a preventive measure, in a case where the child is discovered to have an ability to "swallow the tongue," Butlin advocates that the child should be fed with the breast of either the mother or a wet nurse, if it is possible, and, failing this, with an artificial teat in preference to a spoon.

*Black tongue* has recently attracted considerable attention in the medical journals, and many theories have been propounded to explain its occurrence. Thus Raynaud believed that it was a parasitic affection, and described fine sporules adhering to the filiform papillæ. Armaingaud thinks the black tongue is associated with vaso-motor disturbance, and thinks it analogous to chromidrosis of the skin. Hutchinson is disposed to look upon most, if not all, cases of black tongue as cases of intentional deception. The author thus describes its characteristic appearances:—

"The discoloration is always noticed in the middle of the dorsum of the tongue, usually immediately in front of the V formed by the circumvallate papillæ. It is quite black at the centre, but less so towards the circumference, where it often assumes a light brown hue. The discoloured area is first of small size, but extends, as a rule, very slowly,

until at the end of two, three, or four weeks it covers a large portion of the dorsum. Then it disappears little by little, from the circumference towards the centre, presenting during its subsidence a brownish-yellow tint at the borders of the area. The disappearance of the area is followed generally by desquamation, which commences in the median furrow. The same series of phenomena is repeated as long as the affection lasts . . . from a few days to six weeks or two months."

Butlin, while believing in its parasitic origin, does not consider it due to any special parasite, but that it is an instance of the power possessed by organisms of producing various colours (some of them extremely brilliant).

In the chapter descriptive of the various forms of Glossitis, attention is called to that form of inflammation at the root of the tongue described by Dr. David Craigie fifty years ago as "lingual quinsy." The author considers this is "a severe modification of cynanche tonsillaris, in which the inflammatory swelling is not so strictly limited as usual to the tonsils, but extends to the base of the tongue and neighbouring parts, and produces considerable secondary swelling of the whole of the tongue."

Following on glossitis we find a series of chapters dealing fully and in a clear and encyclopædial manner with Eruptions on the Tongue, Indentations, Fissures, Ulcers, Patches and Plaques, Nodes and Nodules, Smooth Tongues, Atrophy, Hypertrophy, and Cysts.

In this review we cannot hope to call attention to all the interesting points raised in these chapters, and well and fully discussed. The description of Tuberculous Ulcers is especially worthy of attention, and the difficulties of diagnosis between them and cancer is clearly pointed out (p. 104). There are cases in which the author thinks that microscopical examination of a scraping of the surface of the ulcer is of value. We quite agree with the views expressed by the author as to the treatment of tuberculous ulcer of the tongue, which he considers would probably be more successful if the impression were not so strong that the disease must necessarily be fatal, that the ulcer itself is incurable, and that it is useless to do anything for so intractable a disease. He asks would it not be better freely to remove every tuberculous ulcer of the tongue, which appears to be primary, while it is still of small size, and easily within the reach of operation. "I am strongly in favour of this course," he says, "for several reasons—first, because there is the possibility that the operation may preserve the patient from further tuberculous disease by infection through the ulcer; second, because

the disease is in most cases not difficult to reach or to remove, and the operation is far less formidable than that for carcinoma; third, because the ulcer is itself exceedingly distressing, and, through the distress which it occasions, leads to debility and death."

Excision of tuberculous ulcers has been frequently performed, chiefly by Nedopil, but in most cases this has been done under the impression that the ulcers were cancerous. This summary method of dealing with them was the best that could have been devised, both for tubercle and cancer. "If the same decisive method," says Butlin, "were adopted in the case of all doubtful ulcers of the tongue, there would be a striking diminution in the number of deaths from lingual carcinoma" (p. 291).

We turn with special interest to the author's chapters on Cancer of the Tongue, to which he has devoted over 120 pages, and we consider these chapters are perhaps the best in the book, where all are good. He begins by calling attention to the remarkable fact that the tongue appears to be subject to only one variety of cancer—the squamous-celled carcinoma or epithelioma. No part of the tongue is exempt from the disease, but the posterior half is not nearly so frequently affected as the anterior half, and the edges are more subject to it than the dorsum or the under-surface. Again, youth appears to be absolutely exempt from carcinoma of the tongue, and the disease is so rare before the age of thirty that the same statement might almost be made with regard to young adult life. The relation of the disease to sex is as striking as to age. It is incomparably more frequent in males than females. Of 293 cases collected by Barker, 46 only were women. This is attributed to the difference in the habits of men and women, and in the consequences of certain habits, and much is given to support this view. In discussing the predisposing causes of cancer much stress is laid by the author on the so-called "pre-cancerous stage," and in the tongue we have perhaps the best opportunities for observing it. Among his own series of 80 cases the disease is known to have been preceded by leucoma sixteen times, but for want of proper examination, he supposes, the proportion should be vastly greater. In view of the importance of recognising the disease as early as possible, more care should be given in the future to the proper study of those conditions which are known to precede carcinoma, and the sooner this is recognised by general surgeons the more hopeful will become the prospects of extirpating the disease. There is a fatal tendency among too many men "to do what is commonly termed 'give the

patient a chance,' by treating the disease on the assumption that it was syphilitic or simple. Gradually medical men are coming round to the belief that to 'give the patient a chance' means, under such circumstances, to 'give the carcinoma a chance' of obtaining a firm and irresistible hold, and to take all chance of complete recovery from the patient."

There is only one *prognosis* in all instances of lingual carcinoma not operated upon—death. The question then is discussed as to the value of operation as a means of saving or of prolonging life, or of saving pain. First, as to complete recovery. The author assumes recovery to be complete if the patient is quite well and free from all signs of disease a full year after operation. This, we think, is a fair assumption, but in any case there are sufficient instances on record of patients who have been alive and well many years after operation to prove that it is possible to obtain complete recovery. We can point to a case of an old man, eighty years of age, who was alive and well when last seen, two years after removal of one half of the tongue for carcinoma, verified by microscopical examination. Secondly, is life prolonged, and is the patient spared pain and distress in cases even where recovery is not complete? The author considers that if the disease return in the tongue and cannot be removed by a second operation, life is perhaps prolonged; but this can scarcely be considered a gain, as it is only prolonged in order that he may endure all the pain and distress which he would have had to suffer if no operation had been performed. We think the author's view is here rather too gloomy. Our experience is that even when the disease recurs rapidly the pain is not so severe, and in many cases may be absent, probably due to the division of the nerves at the time of operation; so that even under these circumstances operation has been a distinct gain.

"But if the disease does not recur within the month, and the patient dies from the effects of secondary affection of the lymphatic glands, there can scarcely be any reasonable doubt that the manner of death is less painful and hideous, and that, in the large majority of cases, life is prolonged for several months. The gain of life by operation is variously estimated by different authors at from five to eight months."

The treatment is divided into the treatment of the pre-cancerous conditions which threaten to become cancer, and to treatment of the disease when established. Under the first heading the reader is urged to apply nothing but unirritating substances. "Avoid caustics; and again I say, *avoid caustics*. If there is one means

more certain than another to transform a simple into a cancerous sore it is the use of caustic." This is a warning not uncalled-for, as medical men are too prone in doubtful cases to adopt the very opposite practice. The treatment for the disease, when recognised as cancer, is, unless too extensive, removal. Though numerous methods are referred to, the author selects three for description which he considers the best. First, Whitehead's scissor operation; secondly, Marrant Baker's method, in which he begins by splitting the tongue, described in the *British Medical Journal* (1883, Vol. II., p. 765); and thirdly, the Galvano-cautery method, more directly associated with the name of Bryant, and described by him in the *Lancet* (1874, Vol. I., p. 291). In addition to these, operations requiring more room or more extensive measures are described. First in the list is the method of slitting the cheek, "sometimes attributed to Furneaux Jordan, but which I believe is properly due to Gant in this country." The author is aware that this method was a recognised proceeding in Ireland years before either of these gentlemen practised it, being known here as Collis' method, but the fact is not mentioned except in a list of operations at the end of the chapter.

Kocher's operation, which includes a preliminary tracheotomy and the ligature of the lingual artery on one or both sides, as well as the facial artery, is justly praised. The operation is remarkable also as being the only method in which Listerian principles are rigidly carried out. Rignoli's, Billroth's, and Symes' operations receive full recognition.

Great stress is laid on the importance of the after-treatment, and the views held by Whitehead, Marrant Baker, Kocher, Billroth, and Woelfler are all discussed. There is too much truth in his remark that "the German surgeons have taken more pains than the English, or, so far as I am aware, than the surgeons of any other nation, in attempting to fulfil the conditions which are most to be desired in the after-treatment of amputation wounds of the tongue, and their efforts have certainly been attended with success."

We trust we have in some measure shown the general style of the work before us, and we hope that we shall induce surgeons reading this notice to refer to the work itself for fuller information. It is eminently a practical work, and replete with information which will prove of almost daily use to medical men. Let us add that the book contains numerous well-executed and graphic plates of various affections of the tongue; that though so complete on the subject of

which it treats, it belongs to a series of manuals, and, unlike most books bearing that title, it can not only be easily held in the hand, but will readily fit into any ordinary coat pocket; lastly, that it is printed on good paper, in clear type, and is written in clear, well-expressed, and readable English.

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*Cholera Curable: a Demonstration of the Causes, Non-contagiousness, and Successful Treatment of the Disease.* By JOHN CHAPMAN, M.D., M.R.C.P., M.R.C.S.; late Assistant-Physician to the Metropolitan Free Hospital, and late Physician to the Farringdon Dispensary. London: J. & A. Churchill. 1885. Pp. 127.

THERE is too much reason to fear that an epidemic of cholera will be upon us this year or next. If it comes, we earnestly hope that Dr. Chapman's method of treatment may receive a fair trial. So unfortunate, at best, have been the results of our attempts to deal with this dread disease that we gladly welcome any suggestion which offers a hope of reducing its mortality. Some of Dr. Chapman's views of cholera are untenable, and some are wild. The theory on which his method is based may be unsound; but the success of his treatment, in the few cases in which it has been applied, has been apparently so remarkable that he has established a claim to fuller trial when opportunities occur. In 1865 and 1866, 41 cases were treated by the application of ice to the spine, most of them at Southampton. Full details will be found in Dr. Chapman's earlier work, published in 1866, "*Cases of Diarrhœa and Cholera*;" and to this work he confidently appeals in support of his assertion that 14 of the 41 "were so inadequately or so improperly treated that whoever is intent on forming a just estimate of the value of the remedial method in question must ignore them." Even of these fourteen, "*eight were recovered from collapse.*" The total mortality, irrespective of deduction on this ground, was only 41·5 per cent., not an unsatisfactory result. Without these 14 cases 27 remain, "in which the treatment may be said to have been fairly carried out." Of these 24 recovered. The mortality was only 11 per cent. In 1884 Dr. Chapman was so fortunate as to obtain full control of 12 cases at the Hôpital de la Charité, in Paris. The details of these cases are given in the volume under review. Two only proved fatal, giving a mortality of 16·6 per cent. To the objections that "the cholera which prevailed in Paris during November and December, 1884, was of an



especially mild type, and that the cases received into the Hôpital de la Charité were less grave than those received into the other Paris hospitals," Dr. Chapman replies by adducing the following facts. Goodeve states the rough average of cholera mortality at something less than 50 per cent. In 1832, in England, it was 47; in 1848-9, 45; in 1853-4, 46; but in Paris in 1884 the cholera death-rate was 54·47,\* higher than that at Toulon, Marseilles, or Nantes. Secondly, the Charité Hospital cholera mortality in November and December, 1884, although lower than the average, was 48 per cent.; higher than the English averages, and not indicative of special mildness of type. Dr. Chapman affirms that his cases were "quite as severe as those of which 48 per cent. proved fatal." The final result of the two sets of cases is a mortality of 12·8 per cent.

It is a mysterious law of therapeutics that every new method of treatment is wondrously successful at first, especially in its inventor's hands; but, all due allowance made for this, the result of Dr. Chapman's plan has been such as to justify our earnest hope that it may be thoroughly tried on a larger scale. If it helps us to reduce the mortality of this appalling disease we shall not be over-anxious to fit a theory to the practice. We owe it to the author, however, to state, as briefly as possible, his views as to the "proximate cause or essential nature of cholera," referring to Section III. of this work for fuller details. In his own words: "*All the symptoms of cholera are due to simultaneous and abnormal superabundance of blood in, and excessively preternatural activity of, both the spinal cord and the sympathetic nervous centres.*" Of 38 characteristic symptoms enumerated in the larger work, 21 are attributed to morbid activity of the spinal nervous centres. All the abdominal secreting organs are stimulated to excessive action, including mucous and serous membranes. Hence the copious evacuations, containing in abundance perfect epithelial cells. To the same exaggerated nervous influence are attributable the increased internal temperature of the algide stage, sometimes exceeding 105° F.; the renal congestion and albuminuria; and the cramps, internal and external. Other symptoms, grouped under the term "*deprivation*," are attributed to excessive activity of the sympathetic, constricting the arteries. Such are the enfeeblement, without perversion, of the mental faculties, and other effects of cerebral anæmia; arrest of lachrymal, salivary, hepatic, and renal secretions; impeded pulmonary circulation, stoppage of blood-supply to the skin, producing the well-known

\* Correctly, 54·39—564 deaths in 1,037 admissions.

phenomena of the algide stage, and finally death, first of the voluntary, and subsequently of the involuntary, muscles. The so-called "typhoid stage," so common, and so dangerous during reaction, is due to retention in the blood of sugar, bile, and urinary constituents, which the depurating organs, incapacitated by diminished or excluded blood-supply, have been unable to excrete.

The application of cold along the spine by means of the ice-bag reduces the amount of blood in the cord and in the ganglia of the sympathetic; but it is obvious that it may not only correct the hyperæmic condition on which, according to this theory, the choleraic symptoms depend, but go farther and induce a scarcely less dangerous anæmia of these nervous centres. The treatment, therefore, demands the most careful watching, and detailed directions are given in Sect. XI.

It is no necessary inference from Dr. Chapman's theory that cholera is not contagious. The hyperæmia of the nervous centres may, for all we can tell, be the effect of a specific poison—may even be produced by a bacillus. Nevertheless, he is a zealous non-contagionist, and a good many pages of this little book are taken up with anti-contagion polemics. Content with the fact that the overwhelming majority of Indian practitioners are convinced that cholera spreads from man to man and from place to place mainly by human intercourse, we shall not enter into the controversy here. We must, however, protest against the *argumentum ad verecundiam* involved in thrusting forward the "Government of India" as a champion of non-contagionism. The "Government of India" in this controversy simply stands for one "medical counsellor," whose advice, once he had been appointed Sanitary Commissioner, the Government felt bound to accept. Dr. Chapman's use of the plural number in the following passage is altogether misleading:—"The Government of India, . . . advised by its medical counsellors, whose minds are most informed by experience of the matter, and who therefore may fairly be presumed to be the most competent judges, answers it [the question, Is cholera contagious?] in the negative. In thus advising the Government of India, Anglo-Indian physicians rely mainly on experience," &c., &c.

In discussing the "remote, predisposing, and exciting causes of cholera" (Sect. IV.), our author makes the extraordinary assertion that "solar heat of a certain intensity induces cholera." Dr. Chapman must know that intense heat prevails in Australia, in Africa, in Southern and Western and North-Western India, without

inducing anything even remotely resembling cholera. He does not assert that heat is a predisposing cause, but that "heat alone suffices to produce in the nervous centres, *directly*, that condition of superabundance of blood associated with great excitement which is the immediate cause of the production of cholera" (p. 28). It is amazing to see such an assertion in print. A little further on we find another astonishing dictum:—"Alcoholic drinks are powerful aids in producing cholera" (p. 37). On this head it is sufficient to quote Parkes:—"Intemperance, *per se*, has no influence, and teetotalism does not guard against cholera."

In the fifth Section, devoted specially to demonstration of the non-contagiousness of cholera, Dr. Koch's views are assailed with more vigour than discretion. We are told that animals—"dogs, cats, hares, birds of different kinds, and even fish"—do suffer from cholera; that Drs. Lewis and Cunningham "conducted researches for the discovery of a specific organism in connection with cholera," and did not find one (which is quite true); that the comma-bacillus is found in all sorts of places besides the intestines of cholera-patients; and that it is even possible that this little vegetable has been cruelly maligned, and is really beneficial by "promoting and completing the all-important function of digestion." We do not care, however, to point out Dr. Chapman's errors; or to refute the theories on which his practice is based. It is with this that we are concerned. So far as it has been tried it offers promises of success. We welcome any aid in our coming struggle with a formidable enemy, and we conclude, as we began, with the expression of an earnest hope that Dr. Chapman's method of treatment may be fully and fairly tried.

*An Introduction to Practical Bacteriology, based upon the Methods of Koch.* By EDGAR M. CROOKSHANK, M.B. (Lond.); F.R.M.S.; Demonstrator of Physiology, King's College, London. Illustrated with Coloured Plates and Wood Engravings. London: H. K. Lewis, 136 Gower-street, W.C. 1886. Pp. 249.

THIS book will, unless we are much mistaken, prove a real treasure to all students of the science of bacteriology.

After a very full description of the apparatus, material, and re-agents employed in a bacteriological laboratory, the author describes the methods of examining liquids, tissues, &c., and the means of recognising micro-organisms. Then follow the methods of isolating

these micro-organisms from such liquids, tissues, &c., and of carrying on pure cultivations in nutrient media. Next, there is an account of the experiments which have been made upon the living animal by the inhalation of micro-organisms, their administration with food, and by cutaneous and subcutaneous inoculation. The first part concludes with a chapter upon the examination of animals experimented upon, and the methods of isolating micro-organisms from the living and dead subject.

Part II.—systematic and descriptive—opens with a “History of our Knowledge of Bacteria.” After a review of the various classifications the author adopts that of Zopf (“Die Spaltpilze,” 1885), who has warmly supported the pleomorphism of bacteria.

Thus the book includes a systematic sketch of the genera and species of micro-organisms, as well as a description of the methods employed in the investigation of their life-histories. The illustrations—which are numerous and well-executed—consist of thirty coloured plates (with 102 figures), and forty-two woodcuts. In the appendix a descriptive list of important yeasts and moulds is given with any special *technique* required in their case.

No fault can possibly be found with the paper, printing, or price of the book, while its intrinsic merits will make it a valuable acquisition to all who have any practical interest in the subject it deals with.

*A Code of Medical Ethics: with General and Special Rules for the guidance of the Faculty and the Public in the complex relations of Professional life.* By JUKES DE STYRAP, M.K.Q.C.P., &c.; Physician Extraordinary, late Physician-in-Ordinary, to the Salop Infirmary; Consulting Physician to the South Salop and Montgomeryshire Infirmaries; Founder of, and late Honorary Secretary to, the Salopian Medico-Ethical Society, and Shropshire Ethical Branch of the British Medical Association. Second Edition, revised and enlarged. London: J. & A. Churchill. 1886. Pp. 95.

WE are glad to see a second edition of this useful work, careful study of which we recommend to all the younger (and some of the elder) members of our profession. The correspondence columns of the weekly medical papers illustrate abundantly the need of some such manual as this, and Mr. de Styrap's code, occupying the latter half of this little book, admirably supplies the want. Even t

homiletic portions—the Address to the Profession and the Introduction to the Code—which are somewhat platitudinarian in character, may have their uses; as, it must be presumed, the average sermon hath. The code is compiled from the laws of various medico-ethical societies, and from the Code of Ethics of the American Medical Association (1847); and, after careful perusal, we are able to recommend its adoption, both by Societies and individuals. The present edition contains much new matter; including, in an appendix, a discussion of the vexed question of bulletins.

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*The Student's Guide to the Practice of Medicine.* By M. CHARTERIS, M.D.; Professor of Therapeutics and Materia Medica, Glasgow University; formerly Physician and Lecturer in Clinical Medicine, Glasgow Royal Infirmary. London: J. & A. Churchill. 1885. Pp. 566.

As a proof of the popularity of this little book—one of Messrs. Churchill's Student's Guide Series—we may mention the fact that since the year 1878, when we last had occasion to notice it in this journal, two new editions have appeared, while the number of pages has increased from 283 to 566.

We have here in a concise form a selection of the most important facts connected with the ætiology, pathology, symptoms, and treatment of all the diseases which are included in the standard works on medicine. After a few remarks on morbid states and processes, irrespective of the part affected—hypertrophy, atrophy, inflammation, &c.—the author takes up the various forms of disease, treating each with more or less conciseness. Of course, in a book of this size, many diseases are passed over with a few words; but, whenever the author allows himself a little more liberty in this respect, we are much pleased with his clearness and preciseness. We would especially refer to his article on the treatment of typhoid fever, where the appropriate remedies for the various symptoms and complications are discussed at considerable length. Different kinds of antipyretic treatment are described, the new drug antipyrin in rather large doses—30 grains hourly for three hours—being mentioned with approval. We are surprised, however, to find in the account of this disease no allusion to the enlargement of the spleen. A useful table giving the length of the incubation period and the most important dates connected with the eruption in the

most prevalent fevers is introduced. We notice with much approval the introduction of diagrams and engravings illustrating some cardiac and pulmonary phenomena; but in the figures of a typhoid and a tubercular ulcer of the intestine there is, unfortunately, much room for improvement. The chapter which has, perhaps, suffered most from brevity is that on the Diseases of the Nervous System—the whole subject of sclerosis of the brain and spinal cord, for example, being dismissed in one page with the remark that “this subject is still in its infancy.” Throughout the book constant reference is made to a useful list of prescriptions at the end, where the student who is new at the art of prescribing will find some valuable hints. There is a short appendix on the method of performing *post mortem* examinations; a therapeutical index; and a statement of the leading principles to be followed in writing prescriptions in Latin, with some examples, which will prove of service to those students who are expected to write in that language their prescriptions at examinations.

On the whole, we think Dr. Charteris has succeeded as well as anyone could hope to succeed in the attempt to write a useful guide to the practice of medicine of such a small size. To those who desire brevity above all things, we can heartily recommend this book.

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*Rome in Winter, and the Tuscan Hills in Summer: a Contribution to the Climate of Italy.* By DAVID YOUNG, M.C., M.B., M.D., L.R.C.P.E., L.R.C.S.E.; late Professor of Botany in the Grant Medical College, Bombay; Fellow of, and late Examiner in Midwifery to, the University of Bombay; and Fellow of the Obstetrical Society of London. London: H. K. Lewis. 1886. Pp. 288.

WE can scarcely regard Dr. Young's little book as “a contribution to the climate of Italy,” but it gives us some information on that subject, and, to a considerable extent, it fulfils its purpose of vindicating the character of Rome from the suspicion of unhealthiness under which the Eternal City labours. There is a want of a work dealing with the sanitary condition and climate of Rome and other Italian cities to which tourists and invalids resort; but a few “chapters, written during the leisure of a brief summer holiday,” do not supply the need.

Dr. Young accepts the description of the Roman climate as dry,

but having the softness of a warm and moist atmosphere. The annual mean temperature is  $59.5^{\circ}$  F.;  $98.2^{\circ}$  is the highest temperature recorded of late years,  $21.2^{\circ}$  the lowest. The means of the different seasons are—winter,  $45.3^{\circ}$ ; spring,  $57.3^{\circ}$ ; summer,  $74.5^{\circ}$ ; autumn,  $61.2^{\circ}$ . The annual rainfall is about 29 inches, falling on 114 days, nearly one-half of it in autumn. The mean humidity for the year is 66.6 per cent.; in summer, 39. Except for one fact—that in winter and spring the temperature is liable to great and sudden alternations—the meteorological conditions of Rome are favourable to health. The two great medical hospitals of Rome are the Santo Spirito for males, and the San Giovanni for females. In the former, in 1877, 17,453 cases were treated; of these 71.8 per cent. were the result of malaria; 5.6 per cent. were acute pulmonary diseases. In the latter the admissions were 4,181, of which 35.8 per cent. were due to malaria, 4.2 per cent. to acute lung affections. The women being, for the most part, exempt from agricultural labour, are less exposed than the men to malaria, but suffer almost equally with the men from acute lung diseases. The other great facts deducible from examination of the imperfect statistics available are the prevalence in Italy, and particularly in Rome, of acute diseases, and “the absence of so many chronic diseases due to degenerative changes, which are so numerous in our homelands.” In Rome, Bright’s disease, diabetes, hepatic disease, and the chronic results of alcoholic excess are rare; and the inference is a fair one, that such affections may be benefited by a timely change to the Roman climate.

Enteric fever is the chief terror of the English tourist or resident on the Continent. Dr. Young gives us figures which show how Rome stands in this respect:—

“The mortality from typhoid (he says) in 1875 was 238, or 1 to every 1,089 living; in 1880 it was 193, or 1 to every 1,570 persons living; and in 1884 the numbers were 114, or 1 death to about every 2,631 living. In London, out of every 100 deaths from all causes, a proportion of about 1 is due to typhoid fever; whereas in Rome the proportion is nearly 2—but, although higher than London, no other capital in Europe can show a lower death-rate from typhoid than Rome. . . . As compared with Rome, no other large city in Italy has anything like the immunity from typhoid mortality which she enjoys. Florence is nearly double, and Palermo two-thirds more than, that of Rome—so that, throughout Italy, the fever, which of all others is dreaded by travellers, is found to cause a much larger proportion of deaths than it does in

Rome; while among the great cities of Europe generally Rome occupies a place equal, if not superior, to any of them, in the comparatively small number of deaths from this dangerous malady which occur within her walls. . . . The number of strangers who come to Rome every year is reckoned at about 42,000, and of these, probably, not less than 18,000 are English and American. . . . For the seven years from the 1st October, 1876, to 1st July, 1883, the total number of deaths registered was 109, or an average of less than 16 deaths per annum out of a population numbering at least 18,000. Of these 109 deaths, malaria caused 2, diphtheria 1, small-pox 2, and typhoid fever 21, . . . or 1 death to every 6,000 living; but when we compare the number of deaths from typhoid fever with the total number from all causes, it reaches the high figure of 19 per cent. of the whole. These figures, at first sight, are not a little disquieting, and, were it not for the fact that the total number is so small, they would give cause for the gravest anxiety; even as it is, they require attentive consideration." (P. 93.)

The "Roman fever," which travellers dread, and which has given the city its bad name, is neither malarious nor enteric, but some unknown disease, unrecognised in health reports. There is reason, according to our author, to believe in the existence of a type of fever partaking of the character of both; but it is not peculiar to Rome. Prof. Baccelli has given it the clumsy name of "sub-continuous typhoid"—inappropriate as well as clumsy, because he regards it as malarial only, uncomplicated with the results of "typhoid" poison. Dr. Young rejects the view that it is "a typhoid fever modified by malaria, or a malarial fever modified by typhoid," believing it to be quite distinct from both, and, "in fact, a transformation of a malarial into a continued fever, presenting all the clinical features of typhoid, with the exception of the eruption, and producing pathological changes differing more in degree than in kind." Several illustrative cases are given in the fourth chapter.

At page 280 Dr. Young enumerates the diseases to which the Roman climate is unsuitable. The list includes all cases in which excitement of the circulation is likely to be injurious; some forms of dyspepsia, especially those of nervous or atonic character; all in which there is "a tendency to congestive inflammation;" cases of tubercular consumption, with liability to hæmorrhage; of hæmorrhoids and of functional affections of the nervous system, especially hysteria.

Besides the matters which we have specially noticed, this book contains a most interesting account of the water-supply, ancient



and modern, of Rome. A useful chapter is devoted to rules for the guidance of residents as to habits, clothing, diet, &c. Much information is given on the mineral waters of Italy and on its principal health resorts. On the whole, we are glad to accept this work as an instalment of the medical topography of Italy; and we trust that Dr. Young may have inclination and leisure to complete the task which he has begun.

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*Dental Surgery for General Practitioners and Students of Medicine.*

By ASHLEY W. BARRETT, M.B. (Lond.), M.R.C.S., L.D.S.;  
Dental Surgeon to the London Hospital. London: H. K. Lewis.  
1885. Pp. 83.

IN a very small space the author has given much information that will guide those engaged in general practice when endeavouring to relieve that often prostrating malady, toothache.

That surgeons bearing Her Majesty's commission in the naval or military services, or those who are engaged in colonial or country practice, should be able to afford relief to patients suffering from diseased teeth is a fact that is frequently insisted on by the public, and many members of the medical profession. Yet, how the medical student, in his already over-burdened course, is to find time to attend a dental hospital is a difficulty which has not up to the present been solved. In order to compensate in a slight degree for the want of special instruction at a dental hospital, we would recommend medical students and general practitioners to study Mr. Barrett's little work. In it they will find many useful hints about the extraction of teeth, treating an exposure of the pulp, and introducing a simple filling; though we think that the author might, with great advantage to his readers, include under the head of filling the insertion of an amalgam stopping—an operation which should offer no difficulties whatever to the general practitioner, and would be of the greatest advantage to his patients.

The chapter on the first dentition is very good, and contains many valuable hints about the extraction of temporary teeth, and the necessity of the preservation of the temporary molars by stopping. The dates given by the author for the eruption of the permanent teeth are, in our experience, not correct; they are about a year later than they should be.

The remarks on dental abnormalities are necessarily restricted, but will enable the reader to distinguish in most cases the so-called

mercurial teeth from those bearing Mr. Hutchinson's name. The chapter on irregularity in the positions of the permanent teeth contains some excellent advice as to the choice of teeth for removal where over-crowding exists; also the hints given about the employment of an excavator are well worthy of attention.

Mr. Barrett, in his remarks on caries, does not appear to have kept himself up in the literature of the subject, and we disagree with him on several points, such as the action of the leptothrix fungus and the relative durability of the cementum.

The advice given on the subject of extraction is, on the whole, good, but we think that the teaching in the following sentence is radically wrong—"If it be an upper or lower molar, the tooth should be freely rocked inwards and outwards *while forcible traction is being employed*" (the italics are ours). In our opinion, a tooth should be dislocated—never pulled; the practice of pulling most frequently results in the untoward accident against which Mr. Barrett cautions his reader, "while extracting a lower molar an upper incisor may be broken by the back of the forceps."

Were the defects, which we have noted as occurring in this little work, corrected in a future edition, we should have no hesitation in giving it a hearty welcome as a useful addition to dental literature.

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#### RECENT WORKS ON SURGERY.

1. *Fractures and Dislocations*. By T. PICKERING PICK, F.R.C.S.; Surgeon to St. George's Hospital. London: Cassell & Co. 1885.
2. *Surgical Diseases of Children*. By EDMUND OWEN, F.R.C.S.; Surgeon to St. Mary's Hospital. London: Cassell and Co. 1885.

THESE two manuals, intended for practitioners, are of the series which Messrs. Cassell & Co. have begun to publish. They are uniform in appearance and practically so in size, and in regard to typography and printing they are quite attractive.

1. Mr. Pick's book deals with a very large subject. He had the advantage of having no competitor in these countries who had already produced a monograph upon fractures and dislocations, Hamilton's American work being the only one in the English tongue; but being limited to some 500 pages, Mr. Pick could not hope to produce a book as elaborate as that which has been so long

before surgical readers. Nevertheless, he has done his work exceedingly well. After all, it is easy to make a big book; it is not so easy to make a good one; and bulk is not essential to high quality. The great object of a manual like this is to give such a view of the subject in hand as will be just sufficient in detail and in suggestiveness to inform and help the reader. This is what Mr. Pick does. He does not overburden by references; he does not neglect to note things that are rare, and therefore often puzzling; he does not weary with the enumeration of methods; but he gives what we believe will be found to be of most advantage to the practitioner, who is often out of the way of any other help than he can get in such a book as this.

2. Mr. Owen's manual on the surgical diseases of children is of a different type. It travels over much ground, as its title indicates; but it distinctly fills a place in surgical literature. There are numerous manuals for the physician's use in dealing with children; those for the surgeon are very few and are not largely circulated. The book is essentially a practical one. It deals with the signs and treatment of surgical affections as seen in children, and only lightly touches the more abstruse questions of ætiology and pathology.

We can very heartily commend these books to the profession.

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*Royal University of Ireland. The Calendar for the Year 1886.*

Dublin: Alex. Thom & Co. 1886. 8vo. Pp. 743.

THIS annual publication has appeared somewhat earlier than usual in the present year. It is well edited and published, and contains a vast amount of information. The examination papers, set in 1885 and the beginning of 1886, occupy considerably more than half the volume; they cannot fail to interest all intending candidates in the various Faculties. We observe that no changes have been made since last year in the regulations relating to medical students. The Chancellorship of the University remains vacant since the death of the late Duke of Abercorn, and Dr. Gordon has succeeded the late Dr. Benjamin G. MacDowel as Examiner in Medicine, although the Medical Fellowship, rendered vacant through Dr. MacDowel's death, has not been filled up—why so, it is not easy to say.

## PART III.

### HALF-YEARLY REPORTS.

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#### REPORT ON NERVOUS AND MENTAL DISEASE.\*

By RINGROSE ATKINS, M.A., M.D.; Resident Medical Superintendent, District Lunatic Asylum, Waterford.

##### I. INSANITY IN GENERAL.

*Comparative Statistics of Insanity in England and Wales and Ireland.*—From the recently issued Report of the Inspectors of Lunatics in Ireland we learn that on the 1st of January, 1885, the population of England and Wales numbered 27,500,000, and at the same period there were among all classes of the community, and under every denomination of insanity, 79,700 patients. Similarly, in Ireland, with a population of 5,000,000, there were 14,288 registered insane—so far showing that while there was one mentally affected in every 345 resident in the sister country, the ratio in Ireland stood at about one to 348. The inspectors consider that the actual number of lunatics, or those who at one period of life possessed clear reasoning powers, may be approximately set down at one in every 350, the difference being, for the most part, constituted of epileptics, idiots, and congenital imbeciles. With regard to the proportion of lunacy in the two sexes, a very marked difference is noticeable between this country and England and Wales. Here lunacy may be considered as, *de facto*, equally divisible between both, while in England females are in excess by 15 per cent.

*Increase of Insanity in the United States.*—Dr. Foster Pratt, Kalamazoo, Michigan, says that the proportion of insane to the native white population is 1 to 597; in the Southern States 1 to 660, and in the District of Columbia and Territories 1 in 748; that among the foreign whites the proportion in the Northern States

\* The author of this Report, desirous that no contribution to the subject of Nervous and Mental Disease should remain unnoticed, will be glad to receive any publications which treat of it. If sent to the correspondents of the Journal they will be forwarded.

is 1 in 248; in the Southern States 1 in 283; in the District of Columbia and Territories 1 in 236; that among the coloured races the proportion in the Northern States is 1 in 545; in the Southern States 1 in 1,235, and in the District and Territories 1 in 680; that the average proportion of insanity among native whites in the entire United States is 1 in 618; among foreign whites 1 in 250; and among the coloured races 1 in 1,097; and that the total average for the entire country of all population is 1 in 545 $\frac{19}{100}$ . It is a significant fact that the Northern States, including the District and the Territories, containing about 60 per cent. of the entire population, have 70 per cent. of its insane; and that this 10 per cent. of proportional excess is more than supplied by the excess of foreign insane over the number that would result if the ratio of native insane to native population in the North were made the ratio in the foreign population. To illustrate:—The average ratio of insane to all native population (white and coloured) in the Northern States is 1 to 642. If this proportion be applied to the 5,763,894 foreign-born living in the North, the number of insane resulting among them would be 9,240 instead of 23,989, as it now is—a difference of 14,749, which is more than the 10 per cent. of proportional excess of insane to population in the Northern States and Territories. Admitting that census tables prior to 1880 fail to give the full aggregates of the insane, it will be safe for present purposes to assume that whatever errors there may have been in the totals the error of each census was fairly distributed between the various elements of population; so that the census proportion of insane to each class of population was fair and just. This is assuming much, since the errors in the census tables prior to 1880 would affect the foreign population largely from the lack of intelligence shown by the census enumerators; many of the foreign travelling insane population would escape enumeration. How large such possible errors are may be conjectured from the facts stated by the Illinois States Board of Charities. In 1870 the difference between the number of the insane on the list of the census enumerators and those on the State Board of Charities was 1,377, there being on the lists of the Board taken in 1869 1,434 not on the census list, and 115 names not on the Board list were on the census list, while 953 names were common to both. Dr. Clevenger states (*Med. Journ. and Examiner*, 1883) that he has known of instances where name after name was copied from the Chicago Directory and placed on the census lists as inhabitants of certain

districts in the Territories (!). Such errors of omission and commission seriously vitiate Dr. Pratt's conclusions, which are that—1st, beginning with 1860, the foreign-born population has increased since 1850 nearly 100 per cent., while the foreign-born insane had increased 181 per cent.; 2nd, that at the close of the next decade, in 1870, the total foreign-born had increased only about 30 per cent., but the insane had increased nearly 100 per cent.; 3rd, in 1880 the foreign-born had increased less than 20 per cent., but their insane had increased 150 per cent.

*Race and Insanity.*—Dr. H. M. Hurd (Report, Michigan Hosp. for Insane, 1883-84) says:—The Teutonic races, as a rule, suffer from secondary forms of mental disease, and are liable to be victims of quiet dementia, or to entertain systematised delusions. Epilepsy and parietic dementia are not common among them. The Celtic races, on the other hand, suffer from acute mental troubles, and the terminations of their attacks are largely in noisy chronic mania or irritable dementia. There is about an equal tendency with the Germans to degenerative forms of disease, like parietic dementia, paralytic dementia, or epilepsy. English-born patients are more liable to attacks of acute forms of disease, like mania or melancholia, and to active types of degenerative disease, like parietic dementia or epilepsy. They possess more constitutional vigour, and more frequently recover from mania or melancholia than the German or Irish. If the termination is in dementia there is an increased probability that the dementia will present monomaniacal features, and that mental vigour will not be wholly lost. Native-born patients from native-born parents are more liable to mania or melancholia, and the former is liable to become persistent. In relative curability native-born patients are first, English-born (including Scotch and Canadian) next, Germans next, and Irish last. Of the native-born the coloured, or mixed African and white races, are, almost without exception, incurable, and generally develop epilepsy or some other degenerative type of disease. The mixed Indian and French (half-breed) are more curable than the mulatto, because they possess more native vigour of constitution, and are better suited to our climate. Out of 72 cases of parietic dementia, 33, or nearly 46 per cent., were of American birth from presumably American parentage; 20, or nearly 28 per cent., were of English birth (including Canada); 3 were of English descent, but born in the United State; 5 were Irish, 5 of Irish descent, 5 were Germans, and 1 a Norwegian. On hallucinatory psychosis it is found that

race and nativity have some bearing. On careful analysis of all hallucinatory cases it is found that these have been recorded in a little more than 28 per cent. In natives, however, they have been detected in 23 $\frac{1}{4}$  per cent.; in Germans in nearly 25 per cent.; in English in 27 per cent.; and in Irish in 39 per cent. The proportion of hallucinated patients among the English is, doubtless, relatively increased by classing with them patients of Canadian birth, many of whom are, doubtless, of French or Irish descent. These results vary much from those of Spitzka, but are not as ethnologically thorough.

*The Curability of Insanity.*—The following is the summary of an elaborate paper on this subject from the pen of Dr. Pliny Earle, whose investigations in this direction are already well and widely known. The author shows, by the collocation of statistics, the actual results of treatment at a large number of institutions, both foreign and American, bringing the knowledge of such general results to a later date than that contained in any former statistical essay:—

1. *Cases of first attack: duration less than three months.*

a. Earle's 8,316 cases at 23 British asylums; recoveries, 48·71 per cent.

b. Chapman's 38,283 cases at 46 British asylums; recoveries, 48·72 per cent.

2. *Cases of first attack: duration less than twelve months.*

a. Earle's 10,929 cases at 23 British asylums; recoveries, 44·06 per cent.

b. Chapman's 50,409 cases at 46 British asylums; recoveries, 43·79 per cent.

3. *Not first attack: duration less than twelve months.*

a. Earle's 4,768 cases at 23 British asylums; recoveries, 55·37 per cent.

b. Chapman's 19,574 cases at 46 British asylums; recoveries, 53·61 per cent.

In neither of the three foregoing classes have we any American statistics, because our institutions in the tabulation of their cases make no discrimination which would render such a classification possible.

4. *All cases of duration less than twelve months.*

a. Earle's 15,697 cases at 23 British asylums; recoveries, 47·49 per cent.

b. Chapman's 69,983 cases at 43 British asylums; recoveries, 46·52 per cent.

c. Earle's 8,063 cases at 15 American institutions; recoveries, 38·59 per cent.

5. *All recoveries calculated on all admissions.*

a. Chapman's 93,443 cases at 43 British asylums; recoveries, 37·95 per cent.

b. Earle's 33,318 cases at 39 (15 + 24) American institutions; recoveries, 29·15 per cent.

c. Earle's 23,052 cases, third period, at 20 American institutions; recoveries, 29·91 per cent.

d. Earle's 14,372 cases, in one year, at 58 American institutions; recoveries, 27·88 per cent.

The most important general conclusions to be derived from the statistics included in this paper are—first, that the old claim of curability in a very large majority of recent cases is not sustained, and that the failure to sustain it is more apparent and more striking than at any antecedent time; and, secondly, that the percentage of reported recoveries of all cases received at the hospitals in this country (America) still continues to diminish.

It is believed that this diminution is in part to be attributed to the admission of a larger proportion of chronic cases and of cases of greater degeneracy from their origin; in part from the increasing, though, as there is good reason to believe, still far from universal practice of not reporting *as recoveries from insanity*, either mere restorations from a drunken debauch, or forced temporary suspensions from habitual intoxication; and in part perhaps from the adoption of a higher degree of improvement as the standard or criterion of recovery. It may be that there is still another cause of the diminution. Drs. Bucknill and Tuke, in their treatise on Insanity, mention what they call "cooked" statistics. It is possible that in the United States this class of published results is decreasing, and that the reported statistics are more generally given to the public in the spirit of a conscientious loyalty to scientific truth. In conclusion, Dr. Earle expresses the hope that the time is not far distant at which the American Association of Superintendents



(before whom the paper was read on the 16th June, 1885) will so perfect its statistical system as to make a distinction between persons and cases, and enable the reader to learn how many of the reported recoveries are first recoveries, and how many subsequent to the first. This improvement was made in the Massachusetts Statistical Tables in 1879, and in those of the British Medico-Psychological Association in 1883—the latter being now in use in Ireland.—*Am. Journ. of Insanity.*

*The New Nomenclature of Mental Diseases of the College of Physicians.*—The following criticism on the recent nomenclature of mental diseases formulated by the College of Physicians appears in the *Brit. Med. Journ.*, Aug. 8, 1885, from the pen of Dr. Henry Sutherland. In order to estimate correctly the value of this new classification of mental diseases, it is necessary, for comparison's sake, to refer briefly to the original sub-divisions of these disorders, published by the College of Physicians in 1869, and also to Skae's and Esquirol's classifications, to which it is evident the present one is indebted in no small degree.

The following table, where the order in which the diseases were arranged has been slightly altered, so as to bring those called by the same name side by side, will show at a glance the sources from whence the new classification has been derived:—

COLLEGE OF PHYSICIANS, 1869.	COLLEGE OF PHYSICIANS, 1885.	SKAE.
Mania	1 Insanity	Idiopathic Mania
	2 Mania	(a) Sthenic. (b) Asthenic
	3 Hypochondriasis	
Melancholia	4 Melancholia	
Monomania	5 Monomania	
Dementia	6 Dementia, including acquired Imbecility	
General Paralysis of the Insane	7 General Paralysis of the Insane	General Paralysis of the Insane
Idiocy, Imbecility, Puer- peral Mania	8 Idiocy, synonym: Con- genital Imbecility	Idiocy } Intellectual Imbecility } Moral
(a) Connected with par- turition	9 Puerperal Insanity	Mania of Pregnancy
(b) Connected with lac- tation		Mania of Child-bearing Mania of Lactation (and other forms of sexual in- sanity)
	10 Epileptic Insanity	Epileptic Mania
	11 Insanity of Puberty	Mania of Pubescence
	12 Climacteric Insanity	Climacteric Mania
	13 Senile Insanity	Senile Mania
	14 Toxic Insanity, from gout, alcohol, lead, &c.	Metastatic Mania
ESQUIROL.		
Mania		
Lypemania (Melancholia)		

ESQUIROL.	COLLEGE OF PHYSICIANS. 1885.	SKAE.
Monomania	15 Variety (62) Delirium Tremens	Dipseomania
Dementia	16 Traumatic Insanity 17 Insanity associated with obvious morbid change or changes in the brain	Traumatic Mania Sunstroke Mania
COLLEGE OF PHYSICIANS, 1869.	18 Consecutive Insanity from fevers, visceral in- flammations, &c.	Phthisical Mania Syphilitic Mania
Cretinism	19 Cretinism 20 Myxœdema.	

Anyone who has studied the A B C of the subject is aware that all classification must be founded according to the divisions of mind supposed to be affected (as "emotional insanity"), or according to the mental symptoms (as "mania"), or according to the bodily conditions associated with the mental disturbance (as "traumatic insanity"). The committee for mental diseases, very wisely discarding the first of these classifications as impracticable, have arranged these disorders of the intellect under two important divisions—the first including forms of insanity arranged according to mental symptoms; the second according to bodily conditions connected with them. This second division is further sub-divided into those diseases of the mind dependent upon certain periods of life—those dependent upon external causes, and those dependent upon internal causes, which most probably arise from some disease of the nervous system. If these diseases were arranged according to this sub-division, the list would read as follows:—I. *Insanity characterised by Mental Symptoms*—1. Insanity; 2. Mania; 3. Hypochondriasis; 4. Melancholia; 5. Monomania; 6. Dementia; 7. Idiocy; 8. General paralysis. II. *Insanity dependent upon Bodily Conditions*—(a). *Period of life*; 9. Insanity of Puberty; 10. Puerperal insanity; 11. Climacteric insanity; 12. Senile insanity; (b). *External causes*; 13. Toxic insanity; 14. Delirium tremens; 15. Traumatic insanity; 16. Consecutive insanity; 17. Cretinism; (c). *Diseases of the nervous system*; 18. Insanity of brain changes; 19. Epileptic insanity; 20. Myxœdema.

Anyone seeing this classification for the first time must at once come to the conclusion that in it, to conciliate both the disciples of Esquirol and Skae, substantives such as the word "dementia," and adjectives such as "climacteric," have both been allowed a footing. An improvement has been made by inserting the term insanity

instead of mania, so often and so inappropriately used by Skae, and at least half a dozen of his forms of insanity, due to sexual disturbance, have been omitted. Granting the necessity for these forms of insanity, qualified by an adjective, being included in the new classification to satisfy scientific prejudice, it must be confessed that the committee has performed its task with moderation and success. More especially would I direct attention to the two divisions—"insanity from brain changes" and "consecutive insanity"—as being both liberal in principle and including a vast number of cases concerning whose ætiology and pathology conflicting evidence might be adduced. Modern research has necessitated the addition of the term "myxœdema," although much like its brother-disease—general paralysis—the bodily symptoms are chiefly conspicuous.

Having said thus much in praise of the new classification, I may now turn a critical eye upon its deficiencies. In the *Nomenclature of Diseases* of 1869, hypochondriasis was included under the head of "Functional Diseases of the Nervous System," and not, as at present, under "Disorders of the Intellect." Of course the same word may bring different ideas to different minds; but, to my thinking, there is nothing certifiable about hypochondriasis pure and simple as long as it does not glide into melancholia, and surely uncertifiable diseases ought not to find a place amongst mental diseases, if we wish classification to assist us in medico-legal questions. The same remark applies to *delirium tremens*—a disease which often consigns a patient to the asylum, but which nevertheless in itself does not constitute insanity until it passes either into mania *à potu* or chronic alcoholism. I regret to see the term monomania retained. Does it apply to one delusion, or to one groove of delusions, or is it equivalent to exaltation? Again, the would-be note of explanation attached to dementia (I refer to the words "including acquired imbecility") is, I think, misleading. It would be far better to keep the term imbecility for children, and dementia for grown-up people, being consecutive or not upon other forms of insanity. I may make a similar remark upon the explanation attached to idiocy (I refer to the words "synonym: congenital imbecility"). The writings of Drs. Down, Ireland, and Beach, need only be consulted for a moment to discover that idiocy is very frequently not congenital—as, for instance, when it is traumatic, or when it succeeds scarlatina.

Cretinism and myxœdema might surely have been briefly mentioned under the head of mental diseases as well as being included amongst those "not classified."

With these few exceptions, the work has been well done. Both schools—the Somatic and the Mental—have had their claims recognised, and this is something. There can be no doubt that the more simple a classification is, the better for those who are obliged to use it. There are but four states of mind which can be considered abnormal: one of excitement, one of depression, one of exaltation, and one of fatuity, corresponding to the terms of Esquirol:—(1) Mania, (2) Melancholia, (3) Monomania (?), and (4) Dementia. Congenital dementia should be called (5) Idiocy, and non-congenital “Dementia” proper. To these five substantives any adjective the wildest imagination might suggest could be easily added.

In addition, the word (6) “Insanity” should be kept, and also (7) “General Paralysis,”—a disease so strongly marked out by its characteristics as to render it incapable of being classified with ordinary mental disease. Concerning Epilepsy I am doubtful. It is an attendant's term for a certain class of cases; but, considering the fact that it accompanies all the seven forms above mentioned, in many cases it can scarcely be counted apart from them without making the terms overlap. Mania, Melancholia, Exaltation, Dementia, Idiocy, and General Paralysis, combined with a free use of adjectives, ought to satisfy the most pedantic scholar, and it is not certain that the one term “Insanity” would not be sufficient for all practical purposes.

## II. ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

*The Corpus Callosum.*—In a paper read on December 11th, 1885, before the Liverpool Medical Society, Professor D. J. Hamilton expounded his views on the “Significance of the Corpus Callosum.” He differs from most anatomists and physiologists in considering this portion of the brain as not commissural; but, although this theory has been more or less generally held, there are no scientific proofs that it is correct, and the corpus callosum has been, indeed, one of the too numerous hidden secrets of the brain. Dr. Hamilton uses a special method of preparation and of section cutting (*vide Brain*, July, 1883). From extensive investigation it is concluded that a large mass of fibres (called by Hamilton the crossed callosal tract) originates in the cortex at the margin of the great longitudinal fissure, and after running for a short distance with Flechsig's direct motor fibres, crosses over to the opposite hemisphere; but, instead of going to the cortex, turns down to the

inner and outer capsules. They form almost the entire anterior limit of the inner capsule. Hamilton corrects an erroneous impression which became current after the publication of his first papers—namely, that the “motor fibres of the pyramidal tracts decussated in the corpus callosum.” As stated previously, the crossed callosal tract and motor tract (Flechsigs) run together for a short distance, afterwards separating. The crossed callosal fibres end chiefly in the optic thalamus, a few in the caudate nucleus. The mode of investigation of the course of the fibres in the brain by a study of their development, which has led to excellent results in the hands of Flechsigs, has been utilised by Dr. Hamilton, who found that as early as the fourth month of gestation the course of the crossed callosal tract is evident. A case is shortly quoted showing the effect of disease on this tract. In the brain of a woman, aged fifty-three, who had been imbecile from childhood, the first and second convolutions of the right side and the anterior parts of the outer and inner capsules, and of the island of Reil on the left side, were found destroyed; the crossed callosal tract was, as a consequence, absent on the left side, but present on the right. This case undoubtedly lends support to Dr. Hamilton’s views on the course of the fibres. His continued research shows well the advantage of investigating the tracts of the brain from the anatomical, developmental, and pathological standpoints (*Brit. Med. Journ.*, 13th Feb., 1886). In a paper in *Brain*, July, 1885, Dr. Hamilton, in describing a preparation of a foetal brain, stained, after hardening in perosmic acid, says—“The first glance of such a preparation shows that the fibres of the corpus callosum, after crossing, *do not radiate* into the cerebral medulla to reach the cortex of the opposite side. On the contrary, those issuing at the side remain as a *compact ribbon-like band*, which twists upwards, outwards, and downwards, round the ventricular cavity, and ends by entering, or rather forming, the inner and outer capsules by splitting over the lenticular nucleus. It arches much higher up in the embryo than in the adult, because the ventricle at this age is still large, and projects higher up at the sides than when the brain is fully formed. The ventricular cavity has somewhat of a Y shape, the two upper limbs corresponding to the lateral ventricles, while the lower may be taken to represent the relationship of the third. It is round the upper limbs of the Y that the callosal tract turns.

In the October number of *Brain*, Dr. C. Beevor criticises these views of Hamilton. His criticism is based on the examination of

specimens hardened in bichromate of potassium and methylated alcohol, and stained with Weigert's hæmatoxylin, and especially by his acetate of copper method. "From the examination of my specimens," he says, "I cannot find a *single* section in which the fibres from the corpus callosum pass into the internal capsule, and I therefore hold that the accepted opinion is correct—viz., that the corpus callosum is an inter-hemispherical commissure; that the corona radiata does exist as such; and that all the fibres do pass from the cortex direct to the internal capsule. In the drawing which Professor Hamilton publishes in the *Journal of Anatomy and Physiology* for last July, he figures the 'crossed callosal' fibres which he thinks come from the outer extremity of the corpus callosum, and pass upwards, outwards, and downwards to the internal capsule; but his drawings do not demonstrate these fibres actually issuing from or passing into the corpus callosum. They are represented as commencing or ending abruptly in the centrum ovale, away from the corpus callosum. In my sections these fibres are well shown by the microscope, but with this great difference—viz., that these fibres clearly pass on to, or are continuous with, the convolutions of the marginal region of the same side: they course side by side with, and in part pass through, the fibres turning upwards from the corpus callosum. They do *not* pass into the corpus callosum." In this paper Dr. Beevor gives only a short notice of his work in this direction. He hopes to publish a full account in a future number of *Brain*.

*The Optic Thalamus.*—Vittorio Marchi has made an elaborate report of his researches upon the histology of the optic thalamus, conducted in Golgi's laboratory. He finds that the majority of the cells of the thalamus measure 40 to 60  $\mu\mu$ ., although a small number of small cells, 20 to 40  $\mu\mu$ ., are to be found throughout this ganglion. The form of the cells varies greatly, some being spindle-shaped, others pyramidal, others polygonal. All the cells possess numerous protoplasmic prolongations, and one axis cylinder process. The latter, in its course, occasionally is found to divide into several branches, which are lost in the fine network of nerve fibrils, and possibly coalesce with the neuroglia cell prolongations. As a rule, however, each axis cylinder becomes continuous with a single nerve-fibre, and preserves its individuality. The nerve-fibres are united into bundles, which run parallel to one another. The protoplasmic prolongations, on the other hand, by uniting, make up a dense network of fibrils, which surrounds both cells and nerve-fibres.

Marchi thinks it probable that the nerve-fibres and the cells with which they are connected are motor in function; and as their number exceeds that of the cells with branching axis cylinders, he considers the thalamus a motor ganglion. The danger of drawing from anatomical data physiological conclusions has not been avoided by the investigator.

The neuroglia of the thalamus resembles closely that in other ganglia. Marchi finds that the ependyma covering it is formed of cylindrical epithelium, whose roots are branched, and end in the walls of the blood-vessels. He recommends that thalami of the smaller mammals be used to demonstrate the histology of the organ, as they can be well stained with osmic acid. He injects a 2 per cent. solution of bichromate of potassium into the carotid before removing the brain, and thus secures a better hardening of the entire ganglia.—*Neurol. Centralblatt*, July 1, 1885, in *Am. Jour. Nerv. and Ment. Dis.*

*Experimental Researches on the Connection of the Visual Area and the Subcortical Centres and Optic Nerve.*—Von Monakow has been investigating the course of the visual tract by the method of Gudden, extirpating from new-born animals various parts of this tract, and observing the subsequent atrophy. His results are as follow. The first series of experiments was upon rabbits:—

1. If one eyeball be extirpated there follows an atrophy of the optic nerve, and of its continuation through the chiasm, and in the optic tract of the opposite side, the decussation being complete. This atrophy can be traced into the gray masses in which the tract ends—viz., the corpora geniculata externa, the pulvinar, and the anterior corpora quadrigemina. These gray masses are not atrophied in their entire mass, but certain layers only are affected. In the corpora geniculata externa the white capsule which surrounds the ganglion on its lateral, ventral, and dorsal surfaces is reduced to a very thin layer, and the gelatinous substance in the lateral part of the dorsal nucleus is atrophied. The number and appearance of the ganglion cells are not changed. In the pulvinar, the similar portions—viz., the capsular fibres and the basis substance—and not the ganglion cells, are the parts which atrophy. In the corpora quadrigemina anteriora the atrophy is limited to the first two layers—viz., the surface grey and the superficial white layer. In the surface grey the upper zone is reduced by the disappearance of the zonal and ependymal fibres, and of the small cells of this layer—the cells which remain, however, presenting a normal appearance. The

lower zone is reduced by the shrivelling of the polygonal cells which it contains, many of which had lost their processes. In the superficial white layer the axis cylinders of the fibres had almost wholly disappeared, and this atrophy could be followed into the brachium of the corpora quadrigemina anteriora. The other layers of the corpora quadrigemina anteriora were not affected.

2. If the cortex of the occipital convolutions of one side be extirpated, there follows an atrophy of the tract (known as Gratiolet's radiation) which joins these convolutions with the corpora geniculata externa, the pulvinar, and the corpora quadrigemina anteriora. This atrophy can be followed through the posterior portion of the posterior half of the internal capsule and into these ganglia. In the corpora geniculata externa the entire ganglionic mass is much reduced in size, the ganglion cells being the part affected. The gelatinous substance is somewhat atrophied, and the white capsule is thinner than normal, but these parts are not as much changed as in the first set of cases. In the pulvinar both ganglion cells and basis substance are reduced in number and size, and the zonal fibres are less fully developed than on the sound side. In the corpora quadrigemina anteriora the first two layers are not affected to any extent, but a marked atrophy occurs in the deep white layer and in the brachium of the corpora quadrigemina anteriora. The deep grey layer did not appear to be affected, nor were the other layers involved. From these experiments v. Monakow concludes that in the rabbit the visual tract leads from the retina of one side to the occipital convolutions of the other side—a tract which is not direct, but is broken by the interposition of the basal ganglia named. The fact that in both experiments the grey network of the gelatinous and basis substance of the ganglia were affected, while in each, separate parts of the ganglia were atrophied, indicates that the two tracts are not independent. The optic fibres end in the grey network of the ganglia, whence impulses reach the cells of the ganglia, which in turn are in direct connection with the cortical cells.

3. If the portion of the internal capsule through which this visual tract passes be divided, there follows an atrophy both of the Gratiolet radiation and of the cortex. The cortical atrophy was not, however, uniform, but was limited to the third and fifth layers of the cortex—viz., the layers of large pyramidal cells and the layer of multipolar ganglion cells. The ependymal layer, the layer of the small pyramidal cells, and the layer of spindle cells were not involved in the atrophy. Hence v. Monakow concludes that it is



with the third and fifth layers of the cortex that the basal ganglia are directly connected, and that it is in those that visual impulses are perceived.

A second series of experiments has been performed upon kittens. This series shows that in these animals the optic nerves decussate partially—not wholly, as in rabbits. The optic tracts end in the white capsule and in the gelatinous substance of the corpora geniculata externa, in the basis substance of the pulvinar, and in the upper two layers of the corpora quadrigemina anteriora. The occipital cortex is in connection with the ganglion cells of the corpora geniculata externa, the pulvinar, and the deep grey of the corpora quadrigemina anteriora. Extirpation of various parts of the occipital cortex produces various changes in the ganglia, so that the conclusion is warranted that the median portion of the visual area of the cortex is connected with the lateral portion of the basal ganglia, and the lateral portion of the cortical area with the median portion of the ganglia. Hence the fibres from these two portions cross one another in the posterior part of the internal capsule. But, inasmuch as different parts of these basal ganglia atrophy where one or the other optic nerve is divided, it follows that the median half of the occipital cortex is connected with the eye of the opposite side, and the lateral portion of the cortex with the eye of the same side. This is the same conclusion which was reached by Munk in his physiological experiments. The corpora quadrigemina anteriora appears to have less connection with the optic tract in cats than in rabbits, inasmuch as it is less involved in the secondary atrophy. This is also the case in man. That these conclusions are true of man, as well as of the lower animals, v. Monakow is able to prove by means of the examination of pathological cases. In a case of porencéphalie both occipital lobes were destroyed in foetal life by embolism of the post-central arteries. The infant lived two days. After death large defects of substance were found in both hemispheres, and, as a result, a secondary atrophy occurred in the subcortical centres. The appearance was the same as in the extirpation of the cortex in cats. Associated with the defective development of the occipital lobes there was a defective development of the corpora geniculata externa, the pulvinar, and, to a less extent, of the corpora geniculata anteriora. The optic nerves and tracts were also atrophied to a considerable degree. The fact that in both experimental cases and in the case of porencéphalie, the corpora geniculata interna, Luys' body, and the corpora quadri-

gemina posteriora were not affected, is proof that these ganglia have nothing to do with the visual tract.

In a case of thrombosis and softening of both occipital lobes in a patient, seventy years old, who lived four years after the onset of the symptoms, a secondary degeneration was found extending from the atrophied right occipital lobe, through the radiation of Gratiolet into the corpora geniculata externa, the pulvinar, and the corpora quadrigemina anteriora, and thence along the right optic tract to the chiasma, and into both optic nerves. The lesion in the left occipital lobe was of more recent date, and had not produced any atrophy. This case supports in all respects the conclusions reached from the examination of the porencephalic brain and of the animals subjected to Gudden's method of experimentation. It is, therefore, of great value. The results reached may be summed up as follows:—Each eye is connected with both optic tracts, and thus with both corpora geniculata externa, with both pulvinaria, and, to a slight extent, with both corpora quadrigemina anteriora, the connection with the ganglia of the opposite side being more extensive than with those of the same side. The optic tracts end in the grey network and gelatinous substance of these subcortical centres. The ganglion cells of these subcortical centres, into which impulses pass directly from the grey network and gelatinous substance, are in direct connection with the cortex of the occipital lobes, the median portion of the subcortical centres being joined to the lateral part of the cortical visual area, and their lateral portion being joined to the median part of the cortical area. Thus the median half of each occipital region receives impulses from the nasal half of the eye of the opposite side, and the lateral half of each occipital region receives impulses from the temporal half of the eye of the same side.

This conclusion is in perfect accord with that of Wernicke and Wilbrand, reached by a study of pathological cases. It confutes finally the scheme of Charcot, and demonstrates that a lesion of one occipital lobe must produce bilateral hemianopsia.—*Arch. für Psych.*, XVI., 698-750, and XVI., 151-200. Abstr. in *Am. Journ. Nerv. and Ment. Dis.*

*The Tracts in the Spinal Cord.*—Homén, of Helsingfors, has experimented upon dogs to determine the direction, time, and exact character of degenerative changes in the tracts of the spinal cord. By using Weigert's method of staining with fuchsin and hæmatoxylin, it is possible to distinguish the changes occurring in the axis cylinder from those due to degeneration of the myelin, or of

the connective tissue. Homén finds that in all cases the primary change after section of the cord consists of a granular degeneration of the axis cylinder. The axis cylinder appears as a granular light yellow mass, while the myelin is intact, and is surrounded by the normally stained blue erythrophile ring. Later the cylinder becomes swollen, the space between it and the cylinder becomes less; the myelin is involved in the degeneration, and the two cannot be distinguished from one another. Lastly, the appearance of Deiters' cells indicates the participation of the connective tissue and the terminal stage of the process. Homén finds distinct evidence of the beginning degeneration in the axis cylinders three to five days after the operation of cutting the cord. He distinguishes the true secondary degeneration from the traumatic degeneration which occurs near the point of section by the presence in the latter of shining masses of exudation which do not absorb the staining material.

The various tracts do not degenerate simultaneously. The process begins in the posterior columns and attacks their entire longitudinal extent from the first; later, the lateral columns and anterior median column are affected, and last of all the direct cerebellar column. The column of Clarke was thought to be somewhat smaller, the number of fine fibres being decreased, and the number of cells being reduced in one case in which the operation was unilateral. A comparison of these experimental results with the observations upon a number of human cords in which degenerations had occurred has convinced Homén that the process is the same in both. If this is so, his conclusions as to the time, order of progression, and exact histological changes in degeneration are valuable, and should lead to an examination for secondary degeneration in recent as well as in old cases of spinal cord disease.—*Fortschritte der Med.*, 1885, No. 9. *Am. Journ. Nerv. and Ment. Dis.*

*Thermic Sense in Animals.*—M. Herzen contributed to a recent number of the *Revue Scientifique* an account of certain experiments which he made recently on the thermic sense in animals. His observations on man had already led him to the conclusion that impressions of heat are conveyed to the brain by the grey matter of the spinal cord. Animals he found, on the other hand, do not react under the impressions of moderate heat, and when the latter is excessive the reaction is one of pain, not of the specific sensation of heat. On the contrary, however, cold operates on them actively, and M. Herzen succeeded in demonstrating by his experiments that

impressions of cold in animals are really conveyed by the medullary rays which transmit the impressions of touch. The various experiments, which are briefly described in the article, satisfied him that the cortical lesions which destroy sensibility to touch also destroy that of cold, and when the first is preserved the latter always remains; and that in brief, impressions of contact and of cold are transmitted in the same way to the regions of the cortical layers of the hemispheres.—*Provincial Med. Journ.*, Feb. 1, 1886.

### III. NEURO-PATHOLOGY AND PATHOLOGICAL ANATOMY.

*Melancholia and Glycosuria.*—Dr. C. H. Hughes (St. Louis) says that notwithstanding the long-recognised association of hepatic and gastric disorders with depressed types of insanity, dependent often upon coexistent atonic conditions, it is only of late that a somewhat intimate relationship between them and glycosuria has been noted. In some cases the glycogenic function is markedly disturbed—sometimes coexistently, at other times alternately with mental aberration, and more notably in connection with the depressed types, sufficiently often, it may be justly remarked, to justify an examination of the urine in all cases, especially of atonic melancholia, with a view to ascertaining whether the urine be not surcharged with sugar. Enough of such cases have fallen under his observation to justify the suspicion that others might have been found had the urine been tested during the different stages of mental depression, and during the precursory stage of approaching melancholia. He has had under observation a middle-aged gentleman, who, after unusual business strain, was attacked by glycosuria. His skilled and experienced physician found this condition existing. After several months' treatment, though the glycosuria improved, his depression persisted, and became aggravated. Cannabis indica, codein, arsenic, strychnine, and hypophosphites, with electricity, change of air and surroundings, caused both melancholia and glycosuria to disappear; the latter had been most excessive when the depression was least. The gentleman remained robust and well for a year, but is threatened with a return of the trouble. Dr. Hughes believes that this class of cases is far from infrequent, and that glycosuria and melancholia may have prognostic significance. He gives in such cases, disregarding the glycosuria, such treatment as will restore nerve-tone, in melancholia giving preference to sweet and butter-milk and animal and vegetable soups. The condition of the nervous system justifies this plan of

treatment. The condition of the vaso-motor system is an organic justification for disordered hepatic function, and this is why albumen, as well as sugar, is found in the urine of melancholics, even of such as recover. Dr. Hughes is satisfied, from clinical phenomena and results of treatment, that there is an intimate relationship between nervous depression and glycosuria. The influence of agencies that conserve, recuperate, and tranquillise the nervous energies tends to prove this. He has applied galvanism especially to the occipital regions, though he has been better satisfied by a descending cerebral current from the cortex down through the medulla, thence to the dorsal spine, and through the liver, not omitting a gentle systemic séance. It seems probable that, as has been suggested by de Wolf, there are two types of glycosuria, dependent on opposite cerebral conditions.—*Am. Journ. Nerv. and Ment. Dis.*

*On the Alleged Fragility of the Bones of General Paralytics.*—At the meeting of the Antwerp Psychological Congress, held on the 8th of September, 1885, Dr. T. Christian, Physician to the Charenton Asylum, read a paper on this subject, in which he expressed his disbelief in the widely-accepted opinion regarding the occurrence of osteo-malakia as a pathological factor in general paralysis. He remarks that when he, some years ago, denied the existence of this osseous change, Dr. Foville made the following response:—"From the examination which I have made of English literature on this subject, and from investigations which I have been able to control at my own autopsies on general paralytics at Charenton, I have come to the conclusion that the osseous framework of general paralytics undergoes a change which renders it more friable, and that thus, under the simple pressure of the finger, it is often possible to fracture the ribs at the autopsy. It is true," he adds, "that such is not observed in all cases of general paralysis, but is so more especially in those cases which show trophic lesions." In refusing to admit this conclusion and the similar deductions of other observers, such as Davey, Biante, Bonnet, and Vallon, the author is quite willing to allow that in certain cases of general paralysis a softening of the bones has been met with—a veritable osteo-malakia—but the point which he questions is whether general paralysis itself entails an osseous change such as that described, or whether the osteo-malakia is but accidental. In support of the position he takes up, Christian brings forward the following clinical evidence, drawn from his own experience, which appears to him convincing:—"Whosoever," he says, "has seen and has

followed up a case of general paralysis during the progress of the disease will not have failed to recognise the impossibility of preventing the patient falling. During one period of the disease the general paralytic falls at least as frequently and as readily as a child learning to walk, and he falls in just the same way, tripping at the least obstacle, and making no effort to save himself. Now, since I have been in charge of the male wards at Charenton Asylum, there have passed through the wards, during six and a half years, about two hundred and fifty general paralytics. Calculate, I beg of you, the number of falls which these patients will have suffered during this period—falls in the courtyard, falls on the stairs, falls in the dormitories, falls in the gardens—and then say if my infirmary should not constantly show some of these cases with more or fewer fractures! and yet, gentlemen, I can assure you that during these six and a half years I have not come across a single case of fracture. I repeat, not a *single case* in a general paralytic. I must end here. I trust you will come to the conclusion, with me, that general paralysis does not in itself entail any increased fragility of the bones, and that osteo-malakia, when present, is a purely accidental phenomenon, the result of other causes."

At the same meeting, M. Ingels, of Ghent, stated that his personal experience confirmed the statement of Dr. Christian. He had been for more than 25 years in supreme charge of the Guislain Asylum, and had seen but very few fractures, and these scarcely ever affecting general paralytics.—*Journ. Ment. Sci.*, Jan., 1886.

*Double Consciousness.*—At the same meeting of the Congress, Dr. Verriest, Professor in the University of Louvain, showed a "patient with double consciousness." The patient was brought into the room in the *second state*, which is the usual one. In this state she was bright and lively, and expressed herself with exceptional facility in clear and well-chosen language. On the mere *order* being given by M. Verriest, she fell into a sort of cataleptic condition, from which she awoke after a minute or two in a new state of consciousness—the *first state*. On opening her eyes, she was astonished and confused to find herself among so many people; she tried to slip away, and hid her face and eyes. In this new state she, like Dr. Azam's patient, was more serious and melancholy. She had no recollection of anything that had taken place while she was in the "second state," nor did she recognise anyone unless she had seen him in the same "first state." She did not know where she was, how she came to be in that room, how or when she had

left her native village, &c. In the "first state" she was dumb, but communicated with facility in writing. Her writing did not differ from that in the "second state." In the "first state" she could drink, but could not swallow any solid food; in the "second state" the reverse was the case.

The "first state" lasted not more than 15 or 20 minutes, at the end of which the patient fell asleep *spontaneously*, subsequently awaking in the "second state." In this state she then remained indefinitely until, in obedience to *order*—never spontaneously—she returned to the "first state." Only five or six persons had this power over her; the commands of all others were ineffectual. By means of hypnotic passes the patient was thrown into a *third state*, differing from the ordinary hypnotic condition in this—that the patient retained her intelligence and integrity of judgment completely, and that no suggestion had any effect on her. On the hypnotic influence being removed, she invariably returned to the "second state," and had no recollection whatever of the pseudo-hypnotic third state. In the first state she had no more recollection of the third state than she had of the second. Thus she could be made to pass successively and at will through three different states of consciousness, in each of which she displayed a clear and well-balanced intelligence. "In such a case as this," asked M. Verriest, "what becomes of Luys' hypothesis of the functional alternation of the two cerebral hemispheres?"—Rep. in *Journ. Ment. Sci.*, Jan., 1886.

In his recent book, "The Blot on the Brain," Dr. Ireland has a most interesting chapter on the dual functions of the hemispheres, abounding in illustrations from the annals of hystero-epilepsy and experimental hypnotism. The author makes the following quotation from Bérillon on *double hallucinations*:—"It is easy to produce in a hypnotised person hallucinations of sight on one side and hallucinations of hearing on the opposite side. All that is needed is to describe an agreeable picture in the left ear, and to imitate the noise of firing in the right. Immediately the right side of the face expresses fright, while the left still continues to express satisfaction. There exists then, simultaneously in the brain, two hallucinations of a different nature, excited by sounds applied to each ear, each hallucination occupying a different hemisphere; otherwise it would be difficult to comprehend the opposite contractions of the face in connection with each of the hallucinations. As for the critic, who says that the illusions and hallucinations brought on with hysterical

patients in the somnambulistic period are merely simulated by the patients submitted to these experiments, there is only one reply to make—that it is not possible for anyone, even a hysterical person, while in a waking state, suddenly to express joy on one side of the face and fear on the other.”

The inductions of Bérillon are as follows:—

By certain means, we can in man, at the pleasure of the mesmeriser—

1. Suppress the psychical motor and sensory activity of one hemisphere of the brain.

2. Give to each hemisphere a different degree of activity.

3. The two hemispheres having an equal activity, we can create for each of them at the same time manifestations varying in their seat, their nature, and their character—that is to say, the same individual may in the hypnotised state represent by each hemisphere a distinct being, each endowed with its own individuality. Thus, each hemisphere being complete in itself (although in its sensory and motor action it is generally limited to one side of the body), and enjoying within certain limitations its own particular activity, one may say that man, in his mental, motor, and sensory functions, is really *double*; *he possesses two organs* of ideation, two brains. This conclusion of Bérillon's, which the case presented by M. Verriest seems to a certain extent to support, is questioned by Dr. Ireland, who inquires whether “we have not had something like this before to demonstrate theories now as dead as the fancies of astrology?”

M. Verriest, on the same occasion, made a communication on a particular class of paralyses, which he christens “Paralysis through unconsciousness.” The patient, whose pathological condition he described, was paralysed in the right hand as soon as she averted her eyes from it. When she again looked at her hand its power of motion instantly returned, and it executed the most complex and delicate movements. This form of paralysis, of which two or three instances are recorded in the annals of science, was analysed by M. Verriest in the light of its physiological mechanism.

From inductions based on the genesis of these phenomena, M. Verriest was led to conclude that there was a morbid torpor of those cerebral regions on which the image of the paralysed arm was projected—a torpor dissipated under the influence of stimuli coming from the optic centres by way of the associated fibres. Starting from this hypothesis, it occurred to him that stimuli coming along other



associated tracks might perhaps be of equal service in removing the paralysis. He stated that, in fact, mobility returned to the affected limb equally well through the sense of touch. It was enough to make the left hand feel the paralysed right hand in order to restore motion to the latter. The same result was obtained by means of the faradic brush applied with intensity sufficient to cause pain, and therefore consciousness—the notion of the existence of the paralysed hand.

In M. Verriest's opinion, a good number of paralyses in hysterical subjects belong to this category of "paralysis through unconsciousness," with this difference—that the cerebral torpor is not dissipated under the influence of stimuli coming along associated fibres.

Possibly there is a similar production of "paralysis through unconsciousness" in general paralysis, but depending in this case on anatomical changes.

*Influence of Lesions of the Spinal Cord on Convulsions.*—M. Vulpien has communicated to the *Académie des Sciences* the result of researches into the influence which is created by lesions of the spinal cord upon the form of convulsion in experimental epilepsy of cerebral origin, excited by faradisation of the sigmoid gyrus. Transverse section of a half of the spinal cord does not sensibly modify the movements of the right side of the face and right foreleg induced by faradisation of the left sigmoid gyrus. Motor stimulation proceeding from one cerebral hemisphere may be transmitted to the posterior limb of the opposite side, although the spinal cord of the corresponding side may be completely divided in the dorsal region. On the other hand, lesions of the spinal cord modify the effects of motor stimulation proceeding from the brain, so as to replace the convulsions by a contraction of the limb on the side corresponding to the medullar lesion, whereas clonic movements would have occurred if the cord had remained intact.—*Provincial Med. Journ.*, Feb., 1886.

*The Pathological Anatomy of General Paralysis.*—Dr. Franz Tuzek has recently published a memoir on this subject. Exner's new method of demonstrating the tissues of the brain with the aid of the preparations of osmium and ammonium was made use of in the investigation. This method had disclosed the great abundance of the nerve fibres in the cortex cerebri. In the superficial layers these fibres were found to lie horizontally, in the deeper layers to descend vertically. They were found to vary much in calibre—to be largest in the paracentral lobe and finest in the basal ganglia.

Tuczek finds that by the use of dyes, such as fuchsic acid, methyl blue, and ferricyanide of potassium, a differential distinction can be brought out between the nerve fibres of the grey and white substance of the brain. Sahli has indicated the existence of an "erythrophile" and "cyanophile" substance in different parts of the nerve centres, from the varying way they receive colour from fuchsic acid and methyl blue. He has thus been led to deny the existence of naked axis cylinders. It having been assumed that the peculiar functions of the cerebral cortex lay in the nerve cells, scarcely any attention has been paid to the condition of the nerve fibres in degeneration of the brain. Availing himself of Exner's new method, Dr. Tuczek has studied the lesions in thirteen males and four females, who died of general paralysis, paying full attention to the condition of the nerve fibres. These seventeen cases are described at length—the symptoms, progress of the disease, as well as the alterations found after death, being minutely recorded. The result of the whole study is that in general paralysis the primary alteration was found to be a disappearance of "the cortical association fibres" in the frontal lobes. This wasting, as the disease progresses, goes deeper and deeper, and diffuses itself over the whole brain. The disappearance of the nerve fibres is well illustrated by six lithographic diagrams, showing the advanced stages of degeneration. It is especially the finest nerve-fibres which are first affected. Dr. Tuczek compares these association fibres with the bands and connecting-rods of a machine, while the wheels represent the nerve cells. He treats the functional derangements of general paralysis as the result of disturbance of association, from the isolation of the nerve cells following upon the wasting of the fibres. Dr. Tuczek is, however, inclined to rebel against the predominant function assigned to the nerve cells in the maintenance of mental activity. He even quotes, with a degree of approval, Henle's view that in the white substance of the brain we have to seek the organic substratum of the soul's activity. He is disposed to think that what has been described as neuroglia is often composed of nerve fibres, and questions the view which regards general paralysis as an interstitial encephalitis. He observes that characteristic alterations in the ganglia of the brain have not yet been found in general paralysis. In this he is in accord with Westphal and Fr. Schultze. Many things, he tells us, indicate that at the root of this disease we have a primary degeneration of the nerve fibres of the brain. These fibres sustain the associated thoughts and movements most

sorely tried in the struggle for existence. Tuczek confesses that this will not explain all the symptoms of general paralysis. To account for the rapid changes of mood—the passing from exaltation to depression, from mania to melancholia—he is content, like so many other pathologists, to call in the influence of the vaso-motor nerves.

*A Recovery from General Paralysis.*—Dr. Tuczek devotes sixteen pages to the description of the recovery of a patient from general paralysis. This was a man, aged thirty-six, employed in the Post Office. He was dismissed as unfit for duty on the 6th August, 1877, and was received into the Asylum at Marburg on the 27th of the same month. The characteristic symptoms of general paralysis are carefully described, and the progress of the disease and the indications of improvement systematically recorded. He was discharged on the 7th of September, 1878, and on the 13th of the next October the superintendent of the asylum gave a certificate that though he had a remission he suffered from an incurable disease, and was still in a state of mental weakness. In August, 1882, Dr. Tuczek received word that the man was quite well, and had been for a year in the employment of the Post Office. From the last account, dated May, 1884, it appears that though suffering from a swelling of the left foot, he was otherwise quite well and capable both mentally and physically.—*Journ. Med. Sci.*

#### IV. NEURO-THERAPEUTICS.

*Urethan.*—In his report on “Recent Advances in Therapeutics,” in the *Provincial Med. Journ.*, February, 1886, Dr. Macnaughton Jones, writing in reference to urethan, says:—“In a series of articles in the *Practitioner* (Oct., Nov., Dec.), Professor O. Schmiedeberg discusses the pharmacological action and therapeutic application of some etherial salts of carbamic acid, more particularly of urethan, of which he gives a brief summary. This group includes those compounds of the fatty series which have a narcotic action. In slight degrees these bodies produce a condition of diminished excitability and depressed functional activity in various parts of the brain, and insensibility to external stimuli; and, with further increase of their action, sensation and consciousness are depressed, and, finally, narcosis is induced by the complete abolition of the cerebral functions, while reflex excitability is gradually destroyed, and the origins of the nerves for the various vascular districts and organs are deprived of their tone; and, finally, through paralysis of

the corresponding nerve centres, respiratory movements are arrested. Simultaneously, there is a paralysing action of the cardiac ganglia, and an enfeeblement of the heart as a consequence. These are the toxic effects of the group. Any individuality of effect of some of the members which exhibit a deviation from this typical action is due to the fact that certain organs, and especially the vaso-motor centres, are affected earlier and more powerfully by one compound than another. The fundamental character remains the same in all cases. Passing on to the consideration of the special hypnotic under consideration—viz., urethan (prepared by the action of aqueous solution of ammonium on ethyl-chlorocarbonate, which is converted into ethyl-carbamate; this is removed from the neutralised watery fluid by shaking with ether, which is washed in water, and distilled off—the urethan, which remains, being distilled in vacuo, and, finally, dried over sulphuric acid), Professor Schmiedeberg discusses its action on frogs, mammals, and birds. The effects in all three are analogous. Small doses (ten to twenty milligrammes) have little effect on frogs, blunting of the cerebral functions occurring only after doses of twenty or thirty milligrammes, when marked cataleptic conditions are induced, the animal's limbs being retained in any position they are placed in. This is also the case with rabbits, where one gramme is given; while in dogs this cataleptic condition is absent. At first, voluntary motion is impaired, while reflex excitability is retained. In frogs, general immobility ensues, and the most powerful reflex irritability will not disturb it, respiratory movements alone being capable of being produced, and even these are absent after doses of from forty to sixty milligrammes, and 'one might fancy that the animal was dead if the forcible pulsations of the heart did not sufficiently prove that life still continued. . After thirty-six to forty-eight hours, complete recovery usually ensues even from this condition; and, indeed, it is hardly possible to kill a frog by urethan without actually mummifying it with the drug.' In pigeons a like marked and deep cataleptic state is produced by the drug, this being followed by narcosis, so that, 'if we hang the pigeon up by the beak and put the wings in the right position, it cannot possibly be distinguished from a dead one by its appearance.' In rabbits deep narcosis occurs after an average dose of three grammes, and lasts for about two days. The respiratory movements in rabbits are increased both in frequency and depth, due, in all possibility, to direct stimulation of the respiratory centre. The most important fact connected with

the action of urethan is the maintenance of the normal blood pressure under its administration, and this even in deep narcosis, thus offering a strong contrast to chloral hydrate, the latter paralysing the vasomotor centres, while urethan does not affect them. Prof. Schmiedeberg draws these conclusions as regards its action on man: Narcosis for surgical operations cannot be induced by urethan; we take advantage rather of its effects on the general sensibility and mental functions. If given to a healthy man, who has slept well during the night, there will be but little effect, save, perhaps, some heaviness of the head or symptoms of slight intoxication. It is in conditions in which sleep is required after cerebral excitement that it is specially indicated. Too large a dose should not be given at once, as it may cause sickness—rather, small doses, frequently repeated. It does not lessen pain, like morphine. It differs from paraldehyde in acting as a stimulant to the respiration. "Urethan can, therefore, be used in general as a hypnotic, and will frequently be of especial use in those cases where the smell and taste of paraldehyde interfere with its use, and where the action of chloral hydrate on the heart, vessels and respiration must be avoided." The dose is from  $7\frac{1}{2}$  to 15 grains, repeated every two hours. Better, perhaps, two 5-grain doses, repeated at short intervals, in the first instance. It must be regarded in the light of a pure hypnotic, producing physiological sleep, and cannot be expected to relieve pain in the manner of chloral and morphine."

Dr. R. v. Jaksch lately studied the nature and action of this new agent. Urethan is chemically an ethylic ether of carbonic acid ( $\text{NH}_2\text{CO}_2\text{C}_2\text{H}_5$ ), and consists of white crystals freely soluble in water, of a peculiar though not unpleasant taste, and is perfectly odourless. Jaksch, after first having made a number of experiments upon animals (rabbits), by which he ascertained that urethan possesses toxic effects when given in doses of half a gramme to each kilogramme of the weight of the body, used this agent 110 times in twenty different persons with the following result:—When given in doses of one quarter to half a gramme (4 to 8 grains) no hypnotic effect was produced, but when administered in doses of one gramme ( $15\frac{1}{2}$  grains nearly) it invariably caused a sound sleep. It acts principally on the brain, without, however, having any influence upon the peripheral sensory nerves; consequently it proves of no avail against the troublesome cough in phthisis and the pains of neuralgia. But, as it possesses no disagreeable secondary effects, it may be

given where other narcotics are contra-indicated, as in valvular disease or fatty degeneration of the heart, even in the most aggravated cases. The sleep produced is said to be natural and physiological, lasting until morning, and on awakening leaves no unpleasant after-effects. For this reason, v. Jaksch is of the opinion that it will be of special value in the treatment of children, and also for *delirium tremens*, and forms of mania. Urethan may be administered without any corrective, as it is almost tasteless and freely soluble, but for sensitive individuals any excipient may be added. It may be given in the form of powder or solution.—*Alienist and Neurologist*.

Dr. A. S. Myrtle, of Harrogate, records his experience of the drug thus (*Brit. Med. Journ.*, Feb. 20, 1886):—Since October I have been using urethan in a variety of cases with satisfactory results. I have used it in over 50 cases as a sedative and hypnotic, and my experience of its action encourages me to recommend the drug, believing that in certain cases it will prove of great value. The cases in which I have prescribed it were of the usual run of everyday practice, where a sedative or hypnotic was required: General restlessness, sleeplessness, neuralgia, catarrh, certain forms of skin affections with great irritation; also, rheumatism and gout. Many of my patients had some peculiarity of constitution which prevented the use of opiates of the usual type; and it is in this special class that I think urethan will prove of great value. One gentleman, who had suffered from insomnia for weeks, and who cannot tolerate chloral or opium, took 15 grains at bedtime with the most perfect result. He wrote to me and said—"The sleep caused was most pleasant and refreshing. I awoke without a headache, with appetite for breakfast, and, what was equally agreeable, there was no interruption to any of my functions." Similar testimony has been given by the majority of patients, who have taken full doses to produce sleep. In smaller doses its action is less marked; still, it is decidedly calmative and agreeable, causing no unpleasant effect, such as flatulence, nausea, constipation, or headache. It does not affect the nerve centres of circulation or respiration, but spends itself upon the cerebrum. It possesses, therefore, great advantages over the older and valuable sedatives, which have certain evil influences, especially in exceptional cases. Given in gout and rheumatism in full doses, alone or in combination, it has the great advantage over morphia of not interfering with the action of the bowels or kidneys; besides, it is not unpleasant to the taste—the

only objection to it is its price, although that has been reduced 50 per cent. since I gave my first dose three months ago.

*Hypnone* (Phenyl-Methyl-Acetone).—Dr. Macnaughton Jones remarks:—"This new hypnotic (see note by Drs. Dujardin Beaumetz, and G. Bardet, *Nouveaux Remèdes*, Nov. 15, 1885) is a liquid which crystallises below the ordinary temperature (15° to 20° C.) in beautiful white needles, boiling at a temperature of 198°. It has an odour resembling somewhat both that of the bitter almond and the orange blossom. This odour renders the capsule of gelatine the most acceptable form of its administration. Popoff and Neucki have shown that it is transformed in the system into carbonic and benzoic acids, and have detected its final transformation in the urine as hippuric acid. In the adult a dose of from 5 to 15 centigrammes produces sound sleep, and with alcoholics it has hypnotic effects which are superior to those produced by chloral hydrate and paraldehyde; the breath has a disagreeable odour during its administration from the elimination of acetone by the lungs."

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#### ETHER IRRIGATIONS IN THE VOMITING OF PREGNANCY.

MENDEL (*Archiv. de Tocologie*, September, 1885) reports the case of a young woman, pregnant with her first child, of feeble constitution, who had frequent vomiting from the second month of gestation. At the fifth month the vomiting became more persistent, and was accompanied in the interval with nausea, fainting, and general malaise. In a few hours they became so frequent that they succeeded without interruption, producing syncope, absolute prostration of power, noises in the ears, chills, cold and profuse sweats, frequent and filiform pulse. Her life was manifestly in danger. Means the most varied to arrest this vomiting had been employed without result. In their turn antispasmodics had been used (ether, valerian, musk), then opiates, chloral, carbonated and iced drinks, iodine, internally and externally; blisters upon the epigastrium, hypodermic injections of morphine, ether, &c.; ultimately irrigation of ether upon the epigastrium was tried. The effect was instantaneous. A single irrigation sufficed to cut short the vomiting. The patient drew a few long breaths, said she was cured, and felt perfectly well. Later the vomiting returned twice, and each time the ether irrigations arrested all trouble.—*Edinburgh Medical Journal*, March, 1886.

## PART IV.

### MEDICAL MISCELLANY.

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*Reports, Transactions, and Scientific Intelligence.*

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#### ACADEMY OF MEDICINE IN IRELAND.

President—ROBERT M'DONNELL, M.D., F.R.S.

General Secretary—W. THOMSON, M.D.

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#### SURGICAL SECTION.

President—SIR CHARLES A. CAMERON, M.D., President of the Royal College of Surgeons, Ireland.

Sectional Secretary—WILLIAM STOKES, F.R.C.S.I.

*Friday, January 22, 1886.*

DR. MAPOTHER in the Chair.

#### *Keeping Perforation of the Membrana Tympani open.*

DR. M'KEOWN (Belfast) described a method of making an artificial perforation of the membrana tympani which would remain open for a period varying from two weeks to two months. He made a triangular flap of the membrane, commonly selecting either that part in front of or that behind the handle of the malleus. He used a speculum, and illuminated the membrane by the frontal mirror. He then introduced the knife—a very fine straight knife with thin handle—punctured the membrane in front of or behind the top of the handle of the malleus, cut upwards as far as he could; then he made a second incision beginning below, at either the anterior or posterior part of membrane as the case might be, and in a horizontal line with lower end of first incision, and cut upward till the two incisions joined. The flap thus formed, unless in case of very thickened membrane, fell down, or might be turned down with a fine probe, and commonly remained in position, adhering to the membrane below by blood exuded from the cut surface. He had operated on a considerable number of cases—some of long standing—and he gave particulars of some of the most interesting. The ordinary incisions hitherto practised, except to discharge products of inflammation, were of no therapeutic use whatever.



The operation proposed was not for the purpose of making an artificial way for the waves of sound, but to give rest to the ear for a long period, and to induce a nervous and vascular equilibrium favourable to the absorption of deposits and restoration of function. Most troublesome obstructions of Eustachian tubes of long standing had given way to this treatment alone. As to relief of tension in old cases, a simple incision closing in a day or two could do no good, but an opening for a period of two weeks to two months might do a great deal. Time was the very essence of the question from a therapeutic point of view. He illustrated the use of the method in diagnosis, and also in opening a way for efficient treatment of some cases of hypertrophic change of the lining of the middle ear. So far as he could follow his cases, he found none were made worse, some were not benefited, but the majority were relieved in a greater or less degree—some to a remarkable extent. According to authorities, after certain operations on the middle ear, a relapse or even, an aggravation followed; but after this method, in which the fundamental points were a large opening, no destruction of tissue, and the avoidance, as a rule, of all irritation, the improvement had been found sometimes to become even greater after the perforation had closed for some time. He thought the surgery of the middle ear, if they were to make any advance at all, would have to be wrought on new lines and from a new basis such as he indicated.

The CHAIRMAN suggested, from his own experience in other branches of surgery, that if an aperture were made about the centre of the membrana tympani, either behind or in front of the malleus, the cicatrization of the wound below and above might dilate from the centre of the aperture made, and thereby keep the opening of the membrana tympani sufficiently patulous.

MR. W. THORNLEY STOKER said Dr. M'Keown had stated that the old method of incising the membrana tympani was to make the incision behind the malleus, or between it and the incus, and that it was immaterial whether the flap operation was done in front of or behind the malleus; but he had not mentioned to what length he would carry the incision, nor whether there was a likelihood of injuring the chorda tympani nerve.

DR. MAXWELL considered they might borrow from nature a hint as to a method of keeping the perforation patulous. Having regard to the fact that a loss of substance resulted from sloughing, it occurred to him that if the apex of the triangular piece were snipped off the opening would remain longer. He asked how it was that the action of the tensor tympani muscle, when the probe was inserted, could affect the hearing?

MR. FRANKS said Dr. M'Keown's communication was particularly interesting as introducing a new procedure in a class of cases universally acknowledged to be of extreme difficulty. He was not surprised at

hearing Dr. M'Keown state that he had not succeeded in maintaining a permanent opening, but only one that lasted a long time. Dr. M'Keown's method was a very ingenious one, and in his hands had worked wonders. At the same time it was desirable to know what Dr. M'Keown anticipated as the result of maintaining the opening. There were a great many cases looked upon as almost incurable, especially where there was chronic inflammation inside the ear and the joints had become disorganised. How a permanent opening in the membrana tympani could affect those cases he was at a loss to understand. There was also a large class of cases of excessive tension, or where the membrane had been pushed out of position, and in these he could understand the value of a permanent opening, but there were a great many others which he looked upon as hopeless.

DR. M'KEOWN replied.—He aimed at making a free incision without paying any regard to nerves, which must go and take chance. He introduced the knife along the posterior wall, and cut up until the two incisions joined—the apex being above. It would do no good in the world to cut a piece off the tip as suggested by Dr. Maxwell. That plan had been tried over and over again. Poulticing long enough, in the case of a scrofulous subject, would accomplish the purpose, but there was the probability of affecting the mastoid. He would, however, have no objection to try an agent of limited action. In reference to Mr. Franks' observations, he did not intend to convey that those operations were always successful, but, on the contrary, that the statements made by various aurists of the effects of incision could not be substantiated. What he did was to make the incision and let nature operate. Cases in which there were bony deposits were beyond the ordinary remedies, but the surgeon could not always diagnosticate those cases. What he submitted was that it was safe to give his method a trial, and that, with twenty per cent. of successful cases, there was no reason to complain. He treated a man, sixty years of age, who was troubled with noises from dry catarrh, and in that case it was impossible to tell whether the bones were affected or not. He opened both drums, and the man improved slowly in a remarkable way, so as to hear a distance of eight or ten feet, whereas when first examined he could not hear a distance of more than a foot. However, all his perforations closed, and, therefore, he wished to emphasise that his was not a method to maintain a permanent artificial opening, but to keep open a perforation for a period extending from two weeks to two months or more, during which very great changes might take place in the tympanic cavity of the Eustachian tube.

#### *The Radical Cure of Hydrocele.*

MR. M'ARDLE read a paper on the radical cure of hydrocele. After describing the methods of Volkmann, Julliard, and Bergmann, he read

the notes of three cases illustrating the method of incision. He brought forward specimens to show the ease with which the tunica vaginalis could be removed in the operation for excision. He then compared the methods of injection and dissection chiefly as regards—1st. The relative difficulties of the operative procedures; 2nd. The dangers attending each; 3rd. The length of time required for cure; and 4th. The chances of return after each method.

In reference to the third point he called attention to the following as the dates of healing. After—

Injection	Incision	Excision
Billroth . 9th	Volkman . 8-10th	Julliard . 10th
Stoltz . 9-10th	Küster . 14	Bergmann . 11-12th
Weiss . 8-9th	Lister . 17	

The chances of recurrence in various methods he stated as follows:—

	Cases	Recurrences
Injection . . .	315	41
Incision . . .	238	5
Excision . . .	72	0

The percentage of recurrences after injection of different substances seemed to be very much the same. After iodine, 15 per cent.; chloral hydrate, 13 per cent.; perchloride of iron, 11 per cent.; carbolic acid, 10 per cent.; and chloroform, 10 per cent.

Mr. M'Ardle recommended the dissection method in—I. Multilocular hydrocele; II. Hydrocele connected with tubercular testis; III. Cases complicated by mucoid transformation of the cellular tissue of the scrotum; IV. Cases where injection of ordinary hydrocele failed more than once; and V. Those cases of hydrocele in which the tunica vaginalis becomes greatly thickened, coated with chalky deposit, or filled with foreign bodies.

MR. STOKES said he could not, in his own practice, recall a single instance of failure in the radical cure of hydrocele by what Mr. M'Ardle termed the old method of injection, although, however, some of his cases had been injected more than once. In none of the cases was there any serious trouble such as fever, sloughing of the scrotum, erysipelas, &c.; therefore, he was not disposed to alter his method of treatment for one more hazardous. He had seen Volkmann's treatment by incision, and one of the most important points connected with it had not been mentioned—namely, that having incised the tunica vaginalis, and attached the edges with great accuracy to the edges of the skin by continuous sutures, a series of deep sutures were applied to keep the surfaces in apposition. He had seen serious consequences follow the operation—for instance, retention of urine and fever. Another objection was the difficulty of keeping on antiseptic dressings, and there was also that of

preventing the sutures ulcerating through the tissues of the scrotum. While there were all these difficulties and dangers attending the treatment proposed, it was obvious that the old and simple method of using some of the preparations of iodine was the one that in the majority of cases was to be preferred.

MR. BARTON said the question was one of choice between the operations which admittedly had certain risks and the well-established procedure which was without any serious risk. He was therefore gratified that Mr. M'Ardle had recommended those operations for the treatment of hydrocele only in difficult cases which had resisted injection. That limited materially the scope of the recommendation. His own experience was, like Mr. Stokes's, that he could not recollect a single case in which the injection plan had failed to produce a cure, or one in which it had produced dangerous symptoms. There was occasionally delay experienced, but ultimately, with care, those cases were also successful. Therefore he did not see any necessity for the operations recommended. He could, however, imagine complicated cases in which some other means than injection must be tried; but such cases were very few, having regard to the fact that surgeons of experience had not met any. On the other hand, the treatment by injection of iodine was the outcome of many years' surgical experience.

MR. W. THORNLEY STOKER was saved from expressing any material dissent by the saving clause which Mr. M'Ardle employed that the dissecting operation was only to be fallen back upon when other means had been tried. In that sense he endorsed the advice. In a discussion like the present it was well that every surgeon of experience should tell what his observation had been. Speaking from fourteen years' experience as an hospital surgeon, and having operated on a very large number of hydroceles by the injection method, he had no reason to be dissatisfied with it. But he found it somewhat uncertain, and probably his cases of return were more numerous than had been recorded in the practice of other surgeons who had spoken; yet still the number did not discourage him. He had never seen the terrible results follow the injection of iodine which Mr. M'Ardle had described, and which might be read of in books but are not seen in practice—namely, suppuration of the scrotum and necrosis of the testicle. No doubt he had seen inflammation of the scrotum, which, however, he welcomed as an indication that he was going to obtain a cure; and he had also seen a certain amount of inflammation of the testicle, but never of a troublesome character. From his experience he would continue the injection of iodine. He had lately had occasion to resort to another method in a lad, aged sixteen years, who had a double hydrocele of both testicles and both cords. The left he cured by two injections of tincture of iodine, but he failed to cure the right. He then passed a seton into the sac and set up inflammation to effect the radical

cure. The boy left well. Mr. M'Ardle's statistics afforded a strong reason for not resorting to the practice of laying open the scrotum—namely, the occurrence of two deaths for such a comparatively trivial disease as hydrocele.

MR. CROLY, speaking from a long experience—even from his student days—never knew of a single case of hydrocele that resisted the ordinary treatment. Where failure occurred with others he had no doubt it was due to using iodine in too diluted a form. He never thought of using a solution of iodine and water, but threw in a few drops of undiluted tincture of iodine, which invariably effected a cure. What he advised was to dip the probe into a little bit of lard and then wipe with red precipitate. A few grains of red precipitate introduced into the tunica vaginalis, or a few drops of tincture of iodine, would effect a cure in 99 cases out of 100. By this means he would leave nothing to be cut out. He never saw gangrene, erysipelas, or anything bad follow the operation; or if there did, it was the fault, not of the operation, but of the operator.

MR. FRANKS was rather in favour of the operation Mr. M'Ardle had described. He had known many cases of failure following the injection method. Volkmann's statistics recorded 18 out of 22. The one case in which he had himself tried the excision method had taught him a good deal. He found no difficulty in applying antiseptic dressings, and the case proved the most rapid cure and the least painful he ever saw.

SURGEON-MAJOR TOBIN said there was a class of cases not of simple hydrocele, but in which there was some doubt as to the condition of the parts. To such cases the operations of incision described were applicable. If some dressing could be designed that would render the parts aseptic, the operation would be more popular than at present. The ordinary dressing had a hole for the penis to pass through, and that was contrary to Lister's principle.

The CHAIRMAN said it was quite conceivable that with a very large hydrocele, and a thick, rigid sac, it would be hard to procure a cure either by injection or seton. He had himself followed the injection method, varied by trying the seton.

MR. M'ARDLE, in reply, said a great many cases looked upon as cured returned, in spite of all that had been said of the good results of injection. It was far from his intention to recommend incision in all cases, but only in such where the ordinary treatment would not do. He had seen cases in which there were concretions on the inner surface of the tunica vaginalis that could not be treated by injection.

MR. CROLY.—Those were not hydroceles, but diseases of the tunica vaginalis and testicle.

MR. M'ARDLE said they were complicated hydroceles.

The Section adjourned.

SUB-SECTION OF STATE MEDICINE.

Chairman—A. H. JACOB, M.D., F.R.C.S.I.

Secretary—H. C. TWEEDY, M.D. Univ. Dubl., M.K.Q.C.P.

*Thursday, Feb. 4, 1886.*

DR. A. H. JACOB in the Chair.

*Opening Address.*

The CHAIRMAN delivered an address on "The Poor Law Medical Charity System of Ireland." [It will be found in the March Number of this Journal, page 204.]

*Contrast of the Duties of Medical Officers of Health in England and Ireland.*

MR. EDGAR FLINN read a paper on "The Duties of Medical Officers of Health in England and Ireland Contrasted." He referred to the great disadvantages under which the Irish health officer laboured in the execution of his duties, and condemned the system of appointing dispensary medical officers as medical officers of health. The paltry stipends of ten and fifteen pounds a year that they received was an insult to the profession, and it was no wonder that the Public Health Act was not worked, and had become a dead letter throughout the greater part of Ireland. There were few medical officers of health who took any great interest in the sanitary condition of their districts, and those who furnished their boards with monthly reports were unheeded, and but scant notice was taken of their suggestions—in fact, the duties of a medical officer of health in the rural districts of Ireland existed only in name. The difference was apparent in England, where the recognised duty of the urban or rural sanitary authorities was the health of the people; where the medical officer of health was obliged to make monthly or fortnightly reports; these reports were duly discussed, and the officer's suggestions promptly acted upon. The sanitary condition of the towns and villages throughout Ireland showed very plainly to the most careless observer whether the provisions of the Public Health Act were carried out and enforced or not. They spoke volumes. It was yearly becoming more evident that the Irish Local Government Board would have seriously to consider the question of districts and unions being combined for the better administration of the Public Health Act. In lieu of there being such a plurality of medical officers in each district and union, with a superintendent medical officer of health, there should be one or, at most, two who should be men free from the cares of practice, with special sanitary knowledge, and whose pecuniary interests would not be perpetually at war with a fearless discharge of their duties. This com-

bination of districts had worked very well in England, and it was one of the main reasons why the Public Health Act was so thoroughly and efficiently carried out. The same combination of districts should hold good in Ireland. Possibly there might be a difficulty in carrying out a suitable scheme in Ireland, on account of the smaller rateable value; but any combination scheme would be an improvement that would provide for the Irish medical officer of health being independent of the prejudices and whims of local bodies. Improvement was urgently required, and the sooner it came the better for the status of the Irish medical officer of health; it might be in the early future, but it would come some day, he hoped, and dispensary medical officers should either be relieved of their functions as medical officers of health, or they should receive adequate remuneration for the important health duties they were expected to perform.

The REGISTRAR-GENERAL said he was one of the earliest persons who took an interest in promoting the very service which Dr. Flinn had condemned. He had no pecuniary interest whatever in the service—whether it succeeded or whether it failed; but one of the reasons for adopting the present system at the passing of the Public Health Acts, 1874–78, was that, as the English system had worked so very badly, it was thought better to try something else in Ireland. The best health officers had to retire consequent on the authorities letting that system go to pieces. The want of independence of Irish medical officers applied equally in England. If the medical officer was not paid a large salary, or had not private practice, he could not be independent; and if in private practice, there was every possibility of some of his patients being on the Board. It was not usual in England to pay the medical officers of health so well as to secure their whole attention to the duties. But it was impossible that any of the unions in the West of Ireland could supply funds to maintain public health officers independent of private practice. Take, for instance, Westport and Newport Unions, which had been recently amalgamated by the Local Government Board, the ratepayers finding it impossible to maintain separate workhouses, and this was done much to the detriment and inconvenience of the sick poor. The break-down in the case of those unions illustrated what would happen, from Donegal to Cork, if an attempt were made to establish independent medical officers of health for each union. There were reasons why the dispensary medical officer should be also the best officer of health. In a large number of infectious cases the dispensary doctor was the medical attendant, and he was accordingly enabled at once to isolate such cases and stop the spread of the disease. Thus the notification of the infectious disease was avoided, and the necessary precaution taken, without the difficulty experienced in cases of compulsory notification by an independent practitioner. Moreover, the dispensary doctor saw many things dangerous to health as he

went through his district, and these he remedied at once. Hence the advantage of combining the two offices. No doubt the dispensary medical officers were not properly paid as officers of health; but still the combination of the offices rendered the Irish system superior to any existing in any other part of the world. The main object of the College of Physicians and the Irish Medical Association in forcing upon the Government the necessity of superintendent medical officers of health was in the interests of the dispensary medical officers, rather than of the local authorities; but while he did not object to superintendent medical officers of health in large towns, in rural districts the appointment of such officers would prove a disastrous failure. Apart from certain difficulties and occasional mistakes, there was in Ireland the best sanitary system of police that existed in the world.

The CHAIRMAN said he was in the happy position of concurring both with Mr. Flinn and the Registrar-General. The Irish system deserved many animadversions, while, at the same time, he was sensible of the desirability and importance of having the medical officer of health one and the same with the dispensary medical officer. An inspector of nuisances or a superintendent officer of public health could only inspect those places where there was something to inspect. In Ireland there was a corps of 860 officers, who went every day in the week into places where there was most likelihood of the existence of dangers to health. They were their own informants, and therefore he preferred a widely-scattered service, such as the Irish dispensary service, to a concentrated, centralised service, such as Mr. Flinn advocated. But the Irish medical officers were badly paid, and worked under discouragement. The history of the question was this—When the various bodies agitated for a compulsory public health system in 1878, the most essential element urged was the propriety of making the health officer, in the discharge of his functions, independent both of the dispensary committee and the board of guardians; but that proposal was not listened to, and the public health system was constructed on the lines of the best legislation that previously existed in England, and on a system which, if rightly administered, would have produced every hoped-for result. As soon as the Act passed compelling boards of guardians to take the matter in hand, the Local Government Board instructed them, in the first instance, that they were on no account to pay the medical officers sufficient for the discharge of their duties. It was not that the guardians should pay them too much, but that they should not pay them anything like a reasonable competence. Thus the annual salary was preposterously minute, varying from £2 10s. to £5, and being equivalent to a notification that the public health officers were expected to do nothing; while the superintendent medical officer of health was to be a dummy until consulted, and then he was to be paid £1 1s. for the consultation. No encouragement whatever was given by the



central authority for the development of the public health system. Every effort had been made to induce the authorities to carry out the behests of Parliament, and until the authorities and the inspectors saw that the public health officers did their work, nothing could be expected from the public health system ; but if an inspection were provided, and energy infused, a better system than existed in Ireland could not be found elsewhere.

MR. FLINN, in reply, said he bowed to what Dr. Grimshaw had said ; but, at the same time, repeated, from what he had seen here and from his experience in England, that he could not forego what he had stated in his paper. He saw the Public Health Act daily dragged through the mire, simply from the plurality of officers—the sanitary sub-officer, the executive sanitary officer, the medical officer of health, and then the superintendent medical officer of health. Hence it was that nuisances were allowed to exist in places eight, ten, and eighteen months, whereas in England, when a nuisance was reported, he visited the place within three or four hours, and he had leave to act in directing the abatement of the nuisance before his board met. No doubt, the salaries paid to the Irish dispensary medical officers were a disgrace to the profession. In England the combination of districts had been a success. The districts were well managed, and there never was any difficulty.

The Sub-Section adjourned.

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## SUB-SECTION OF ANATOMY AND PHYSIOLOGY.

Chairman—FRANCIS T. HEUSTON, M.D.

Sub-Sectional Secretary—J. A. SCOTT, L.K.Q.C.P.

*Opening Meeting, Thursday, February 11, 1886.*

DR. F. T. HEUSTON in the Chair.

### *Introductory Remarks.*

The CHAIRMAN said, since it was not the habit to deliver an introductory address in this Sub-Section, he would follow the example of his predecessor in the chair, particularly as the only subject which he thought of bringing forward was what Irish anatomists had done in Ireland, and that subject had been already dealt with by Sir Charles Cameron, President of the College of Surgeons, at the opening meeting of the Surgical Section.

### *The Pisi-uncinatus Muscle.*

DR. BROOKS exhibited the pisi-uncinatus muscle, as one of exceptional rarity, which he had discovered in dissecting a hand.

DR. FRAZER, following the remark about the tendinous band in the cat, and knowing what he did about the peculiar action of the larger felidæ, particularly the tiger, in striking not from the shoulder but from the paw alone, suggested that there would possibly be found in those animals more frequently than in man at least a corresponding band of muscle to that exhibited. So, too, with the moles. He asked what other instances were recorded of the muscle in question?

DR. BROOKS, replying, said he had found only three examples.

*Curious Combination of Abnormal Muscles in the Upper Arm.*

The CHAIRMAN made a communication on a muscular anomaly of the right upper extremity (a drawing of which, by Mr. A. J. Cary, he exhibited), which consisted of two sets of muscular fibres, the lower of which passed from the latissimus dorsi across the first stage of the brachial artery, while the upper, taking origin from the cartilages of the 6th and 7th ribs, crossed the axilla and third stage of the axillary artery, to be attached with the former set of fibres into a broad triangular tendon, the external border of which passed over the biceps and pectoralis major to be attached into the deltoid, while from its inferior angle a tendon passed to the internal condyle of the humerus. Dr. Heuston considered the lower set of fibres to be an example of the Achselbogen, while he considered the upper fibres to be an example of the chondro-epitrochlearis, the tendon attached to the internal condyle being the proper tendon of those fibres.

DR. FRAZER said the excellently executed drawing deserved the highest commendation. He asked what was the exact relation of the upper muscle as a homologue to the third pectoral found in birds of powerful flight?

DR. BROOKS considered the chondro-epitrochlearis as a very rare muscle. In two or three sessions of hard work he had not met with a single specimen. A specimen was discovered by Dr. Macalister. *A priori*, they might regard it as tolerably rare, as it was not represented in the lower animals, especially in the monkeys—those which in their anatomical relations were nearest to man—whereas the dorsi-epitrochlearis was a monkey muscle.

The CHAIRMAN, in reply to Dr. Frazer, said none of the fibres bore any relation to the coracoid process. As regards the point mentioned by Dr. Brooks, knowing the greater frequency of the dorsi- than the chondro-epitrochlearis, he had examined to see if the fibres passing down bore any connection with the triceps muscle, as they would do had they been dorsi-epitrochlearis, but he found none. The lower part was continuous with the internal brachial ligament, while there were no connecting fibres with the teres major or the triceps. Therefore, he put the dorsi-epitrochlearis out of the question. There was only one point in

connection with the specimen as to which he had not satisfied himself—namely, the exact nerve-supply. He could find no branch terminating in the fibres.

*Nerve-supply of the Short Muscles of the Thumb.*

DR. BROOKS read a paper on "Varieties in the Nerve-supply of the Short Muscles of the Thumb." He made a preliminary statement, in which he said that his object was to show that, in a large proportion of cases the outer head of the flexor brevis pollicis received a nerve-supply from the deep branch of the ulnar nerve; whereas, in all the English text-books, the outer head was described as being supplied by the median only. Also in Henle's "Nervenlehre" (1879), Gegenbaur's "Anatomie" (1883), and in the section of Krause's "Handbuch" (1880), which is devoted to nerve anomalies, this arrangement was not mentioned.

DR. BROOKS then alluded to the different ways in which various authors had described the thumb-muscles, and quoted the description in the last edition of Quain's "Anatomy," in which the outer or radial head of flexor brevis is shown to consist of two parts—a larger superficial portion and a fasciculus springing from the deep origin of the muscle. He had invariably found that when one of these portions received a supply from the ulnar, the nerve also extended to the other.

DR. BROOKS then said that last Session, finding a case of the flexor brevis (outer head), supplied by the ulnar nerve, and believing it to be very rare, he had shown it to Professor Cunningham; he found that the Professor had notes of a similar case, and also of a case in which the median gave branches to both heads. This Session he had determined to take statistical notes, with the following result:—

Both heads supplied by ulnar alone—5 cases.

Median supplying outer head and giving twigs to inner, the latter also supplied by ulnar—2 cases.

Outer head supplied by both median and ulnar—5 cases.

Outer by median, inner head by ulnar (the so-called normal arrangement)—4 cases.

Thus in 10 cases the outer head received a supply from the deep branch of the ulnar, while in 6 it was supplied by the median alone.

DR. BROOKS was inclined to think that when more extended observations were made, the most frequent arrangement would be found to be—

Inner head—ulnar only.

Outer head—ulnar and median.

The CHAIRMAN had little doubt that Dr. Brooks had satisfactorily proved that the received idea as put forward in the text-books was wrong. In a given session a number of anomalies would occur of a particular sort; but where it was found in ten out of sixteen cases that the nerve-supply of the outer head of the flexor brevis pollicis muscle was from the

ulnar nerve, he could hardly regard so many cases as a run of anomalies, and he hoped, therefore, that Dr. Brooks would continue his investigations until he had completely proved his point.

DR. HENRY KENNEDY said one important aspect of the subject was how far nerve-supply and muscular change accounted for the extraordinary difference that existed in different individuals as regards the use of the hand—for instance, in painting, carving, playing, writing, &c. Again, there was a remarkable difference in the power different people had of facial expression. Thus, Garrick, with the slightest movement, could express the idea he wanted to convey without saying a word. Another important aspect of the subject was that of its bearing on local disease of nerves. Recent investigations indicated that tabes dorsalis, locomotor ataxy, and writers' cramp were not dependent on the spinal cord at all, but on local disease of the nerves themselves.

DR. FRAZER said their knowledge of animals, and of the nerve and arterial supplies, was in a transition state. What they had been in the habit of calling anomalies were now referable to the real causes in muscular anatomy as being the representatives of things in other animals, and under other conditions, pointing to the existence of a law. The elucidation of that law was an important end; and the first step towards it was the record of a fact, while the next was the true interpretation of that fact. Nothing was more likely to throw light on the subject than the dissection which Dr. Brooks had been engaged in—namely, of the hands of apes and some of the lower animals.

DR. BROOKS, replying, said the remarks made by Dr. Frazer recalled to his mind that in dissecting the hand of a monkey (*Macacus nemestrinus*) he had found the outer head of the flexor brevis pollicis supplied partly by the median nerve, and partly by the deep branch of the ulnar nerve. Again, in dissecting both hands of a chimpanzee (in which animal the flexor brevis has no inner head) he found the outer head supplied by the median only. He had simply put forward a working hypothesis as better than nothing at all; but the acceptance of any theory would require a long series of observations not only in man, but an investigation of the mammalian section, at any rate of the vertebrata.

The Sub-Section adjourned.

## MEDICAL SECTION.

President—F. R. CRUISE, M.D.; President and Fellow, King and Queen's College of Physicians.

Sectional Secretary—A. N. MONTGOMERY, M.K.Q.C.P.

*Friday, February 26, 1886.*

The PRESIDENT in the Chair.

*The Causation and Treatment of Pneumonia.*

DR. MARTIN, of Portlaw, read a paper on the occurrence of a large number of cases of pneumonia within a period of three months in a space about equal to half a mile square in the town of Portlaw. In only one instance did two cases occur in the same house, and one of these was seized while working at a place six miles distant, where he had been residing for three weeks previously, and only returned home to be nursed. No person occupied in attending on or nursing a case was affected, even under circumstances particularly favourable to such a result. In almost every instance a certain amount of unsanitary surroundings existed, and to this, aided by the particularly unfavourable climatic influence which prevailed, he attributed the outbreak—the more so, that in almost every instance the patient stated that the illness was produced by a chill caused by exposure to the peculiarly severe easterly winds which prevailed, when heated by exertion. Of the 28 cases 26 recovered. The treatment was based on old-fashioned lines; in the early stage aconite, salines, diaphoretics, poultices, leeches; and in the advanced stage, occasionally flying blisters, and after their removal, cotton wool covered by gutta percha paper—the patient's strength being supported all through by milk, beef-tea, eggs, and occasionally stimulants, but in very moderate quantities. He tried quinine in some cases, and though perhaps with advantage, is not in love with it as a remedy, preferring brandy when temperature runs high, which agrees better with the patient and acts more speedily; but he states, that when one considers the very unfavourable circumstances under which he had to treat these patients in their own homes—seldom clean and amidst extreme dirt and poverty—his opinion on the action of a special drug like quinine cannot be very positive or reliable.

DR. J. W. MOORE remarked that it was interesting to note the coincidence of the prevalence of pneumonia and measles in the Dublin district with the Portlaw epidemics of these diseases last year. While Dr. Martin regarded the infective theory as nonproven, because, save in one instance, no two cases of pneumonia occurred in the same family, yet there was a general history of exposure to chill; and this exposure to chill had been

recently advanced as one of the strongest arguments in support of the infective theory in acute pneumonia. In the *Medical Chronicle* Dr. John Priestley, when summing up the different factors in the ætiology of the disease, included chill as strongly supporting the view of its infective nature. The chill caused a certain amount of bronchial catarrh, enabling the virus of the pneumonia to find ready entrance into the system—in fact, the chill produced a traumatic condition of the bronchial mucous membrane, facilitating the entrance of the virus. The specific theory was also objected to on the ground that the disease sometimes resulted from a blow or injury, rending the lung, and producing a “trauma” by which the virus entered the blood. From his own observations in Cork-street Hospital he concluded that in most cases they had to deal with an essential or true pneumonic fever. The relation of the local lesion to the disorder seemed to be analogous to that of the lesion of Peyer’s patches to the essential disorder in enteric fever. The analogy was singularly complete. That a second case did not occur in the same house told very little against the zymotic theory, because other people might not have been exposed to the chill which produced the “trauma.” Attendants on enteric fever cases rarely contracted that disease. Dr. Grimshaw had, in a paper on the subject, called the disease pythogenic pneumonia—a term which had been accepted in this country and abroad.

The REGISTRAR-GENERAL (DR. GRIMSHAW) said the paper referred to was as much Dr. Moore’s as his, since it comprised some cases from Cork-street and some from the Meath Hospitals. He was not sure whether Dr. Moore or himself had invented the term, but he was strongly of opinion that the disease was produced by pythogenic causes—a view which was strengthened by Dr. Martin’s paper. That chill was the exciting or predisposing cause was very doubtful; on the contrary, he believed the chills in connection with pneumonia were like those spoken of in connection with typhus fever. People said they got typhus fever by getting cold, whereas it was well known they did not, but that the cold was the first sign the patient noticed. In Dr. Martin’s cases the exhalations from the manure and the cesspools were the real causes of the disease. Neither pneumonia nor enteric fever could be said to be very infective in man. On the other hand, veterinary surgeons believed pneumonia to be very infective in the lower animals, spreading among them with great rapidity, and hence the animals affected were destroyed to prevent the extension of the disease. It might be like other diseases which were modified in their virulence in different animals. Since pneumonia was associated with enteric fever, which was one of the reasons for calling it pythogenic pneumonia, he asked were there any cases of enteric fever in the houses referred to by Dr. Martin? As further indication of the causation of the disease, he had met with several cases of persons who had been working in the bed of the Liffey,

where they were exposed to chills and the emanations from the decomposing sewage. Three men were attacked immediately after they commenced work, showing strongly the connection between the disease and that condition. Many local outbreaks of febrile catarrh, called influenza, which were confined to one house, had, he believed, a similar origin.

DR. HENRY KENNEDY did not look upon pneumonia in any form as an infectious disease. In any acute disease rigor was the rule, and therefore he did not give any particular weight to its occurrence in pneumonia. He knew of an instance of nine out of twenty-one cows, all grazing on the same grass at Santry, dying of pneumonia, but the nine were Alderney cows, while the remaining twelve which remained healthy were of a different breed. In those cases he set down the disease to the difference in breed. Again, as to the epidemic character of pneumonia, he remembered, a good many years ago, having within three weeks nearly twenty in hospital in whom the upper lobe of the lung—generally the right—was attacked. The health of animals was no doubt deteriorated by bad sewage, but why the result should be pneumonia at one time and typhoid fever at another was a matter still *sub judice*.

DR. WALTER SMITH said we must still confess to ignorance of what determined the outbreak in a large number of cases. He was struck with the remarkable contrast which the mortality of Dr. Martin's cases presented (only two deaths out of twenty-eight cases) as compared with that of the epidemic in Dublin. In Dr. Martin's cases it was of great practical interest to note that the pneumonia left permanent traces behind, as contra-distinguished from the usual experience of acute pneumonia, which, as a rule, subsided, and the lung in many cases perfectly recovered, the patient being not a whit the worse—excluding, of course, cases of bad pleurisy. If a specific fever, it would influence their views of treatment in the same way as did the pathology of continued fevers. No physician dreamed of stopping or curing one of the recognised varieties of continued fever; and if pneumonia came within the category of zymotic fevers of short duration, hope must be abandoned of directly interfering with its course and we must be content to obviate the tendency to death.

DR. WILLIAM MOORE, referring to the contagiousness of pneumonia, instanced the case of a man, aged seventy, who died of it, and whose attendant, aged twenty-two, was attacked three or four days afterwards with the disease, from which he recovered.

DR. FINNY said investigations subsequent to Murchison's had shown that putrid matter would not of itself produce enteric fever, and that there must be some other factor. Granted that enteric fever and pneumonia were two different fevers, there must be a separate virus to each, and unless the virus could enter into decomposing animal and vegetable matter, he did not think it was right to say that fever in the one

case or pneumonia in the other was produced by such decaying matter. No doubt Dr. Martin had shown that bad sewerage existed, but that was not sufficient to show that it produced pneumonia. Throughout the country the houses were much in the same condition, sewage matter being outside the doors. Again, taking the view that there was a special virus, it carried out the observations of Friedländer of a micrococcus being the cause of the disease.

DR. GORDON having seen many cases of pneumonia, both in hospital and in private practice, was impressed during the epidemic with its extreme contagiousness. In a private house at Blackrock, well circumstanced as to air, ventilation, and sewerage, there were six cases of pneumonia, one after another, at an interval of two or three days, and of these two proved fatal, the remainder recovering after a long time. He was also struck with the rapidity with which the fatal cases died, mostly within 48 hours of attack with the decided symptoms of pneumonia, and what was also remarkable was the rapidity with which a form of purulent infiltration set in through the greater part of the pulmonary system, in some cases the entire lung being engaged. Of the fatal cases, not one went through the ordinary progress of the disease.

MR. DOYLE said he never lost a case where pneumonia occurred with children. The German authorities recommended the treatment of the fever and not the pneumonic complication at all. It was important to determine the physical signs of congestion of the lung, which varied very much. Dulness on percussion was an unreliable sign, and he regarded resistance under the fingers as much more reliable.

DR. J. W. MOORE, in explanation, said there was great confusion as to the term "chill." He never meant by the term the initial rigor of the disease, but the general chill preceding the outbreak and which exposure to the N.E. wind would cause. In 1883-84, of 74 cases of croupous pneumonia in Cork-street Hospital, 9 proved fatal, and in the epidemic year 1884-85 101 cases, of which 24 proved fatal, being a mortality of 25 per cent.

DR. MARTIN, in reply, said that pneumonia was a zymotic disease in a great many cases, he had not the slightest doubt. Some years ago an old man and his wife died in a couple of days, and there were no other cases in the neighbourhood. Seeking a cause, he found under the door an exceedingly foul sewer. Having seen the paper of Dr. Grimshaw and Dr. J. W. Moore, he at once saw an analogy between the cases. He believed that the pleuro-pneumonia in cattle was a zymotic disease, caused by filth and dirt, and not by contagion. He did not look to the treatment as curing pneumonia, but devoted his care to the general support of the patient. At the same time, where the patient was ill with a cough, he gave something to remedy the cough. He emphasised the importance of teaching the young practitioners some of the good old-fashioned methods,



as they often forgot the position of the patient; for instance, in inducing profuse perspiration, by the use of jaborandi, where the patient might be possessed only of a piece of a quilt and a torn shirt, so that the reaction left him worse than before.

### *Hyperpyrexia in Rheumatic Fever.*

MR. A. N. MONTGOMERY, in the absence of Mr. W. Langford Symes, read his paper on the above subject. [It will be found at page 289.]

DR. HENRY KENNEDY said there was no doubt that death was frequently accelerated by the cold bath treatment, and therefore he was glad to see that the author advocated extreme caution and recommended a method which would be comparatively safe.

DR. WILLIAM MOORE believed there was no disease in which hyperpyrexia occurred less frequently than in acute rheumatism. He saw recently a young gentleman with this affection, and the temperature never rose above  $101^{\circ}$ ; and yesterday he saw a case of a lady perfectly powerless, whose pulse was 120, while her temperature was normal. As to temperatures of  $108^{\circ}$ ,  $109^{\circ}$ , and  $110^{\circ}$ , he never saw them. There was a form of rheumatism which for a better name was called pyæmic, in which high temperature might occur.

DR. FINNY said he had met three cases of hyperpyrexia, and Mr. Symes' case made a fourth. As Dr. Moore had pointed out, the high increase of temperature bore no relation to the local manifestation of disease which might or might not be coincident. In Mr. Symes' case the local inflammation of the mitral valve produced the high fever and the nervous symptoms. Still he referred to the possibility of the delirious phenomena and the high fever occurring without any local disease. Hyperpyrexia was a most obscure subject. The practical point of the paper was that it demonstrated a means of bringing down the temperature.

The Section adjourned.

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### A REMEDY FOR CORYZA.

RABOW (*Deutsch. med. Wochenschrift*, 5, 1886) has repeatedly seen benefit from the following powders, used like ordinary tobacco snuff, which also they resemble in appearance:—1. Menthol, 2 parts; roasted coffee, 50 parts; white sugar, 50 parts; mix, and take as snuff. 2. Cocain hydrochlorate, 1 part; roasted coffee and white sugar, of each 50 parts; mix, and use as before.—*Norsk Magazin for Lægevidenskaben*, Marts, 1886.

# SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P., F. R. Met. Soc.

## VITAL STATISTICS

*For four Weeks ending Saturday, February 27, 1886.*

The deaths registered in each of the four weeks in the sixteen principal Town Districts of Ireland, alphabetically arranged, corresponded to the following annual rates per 1,000 :—

Towns	Weeks ending				Towns	Weeks ending			
	Feb. 6.	Feb. 13.	Feb. 20.	Feb. 27.		Feb. 6.	Feb. 13.	Feb. 20.	Feb. 27.
Armagh -	31·0	20·7	41·3	25·8	Limerick -	29·7	39·1	16·2	17·5
Belfast -	28·7	26·3	26·3	26·3	Lisburn -	19·3	9·7	9·7	38·7
Cork -	30·5	24·7	25·3	24·0	Londonderry	41·0	37·4	19·6	30·3
Drogheda	50·7	33·8	8·5	16·9	Lurgan -	46·2	41·0	46·2	15·4
Dublin -	32·8	38·0	31·6	29·8	Newry -	10·5	24·6	42·1	28·1
Dundalk -	30·6	17·5	17·5	34·9	Sligo -	19·2	14·4	24·1	9·6
Galway -	53·8	23·5	33·6	30·3	Waterford -	27·8	27·8	32·4	27·8
Kilkenny	38·1	21·1	38·1	21·1	Wexford -	34·2	29·9	25·7	17·1

In the week ending Saturday, February 6, the mortality in twenty-eight large English towns, including London (in which the rate was 22·3), was equal to an average annual death-rate of 22·2 per 1,000 persons living; in Glasgow the rate was 26·1; and in Edinburgh 18·0. The average annual death-rate represented by the deaths registered in the sixteen principal town districts of Ireland was 31·7 per 1,000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 3·2 per 1,000, the rates varying from 0·0 in Limerick, Galway, Newry, Kilkenny, Drogheda, Dundalk, Sligo, Lisburn, and Armagh, to 15·4 in Lurgan; the 9 deaths from all causes registered in the last-named district comprising 1 each from measles, typhus, and enteric fever. The 122 deaths from all causes registered in Belfast comprise 3 from measles, 5 from scarlatina, 1 from typhus, 3 from whooping-cough, 1 from diphtheria, 1 from ill-defined fever, and 1 from diarrhoea; among the 47 deaths in Cork are 2 from measles, 1 from

scarlatina, and 1 from enteric fever; and the 23 deaths in Londonderry comprise 1 from enteric fever, and 2 from diarrhoea.

In the Dublin Registration District the births registered during the week amounted to 230—107 boys and 123 girls, and the deaths to 228—113 males and 115 females.

The deaths represent an annual rate of mortality of 33·7 in every 1,000 of the estimated population; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 32·8 per 1,000.

Twenty-eight deaths from zymotic diseases were registered, being 7 over the number for the preceding week, but 6 under the average for the fifth week of the last ten years; they comprise 3 from scarlet fever (scarlatina), 2 from typhus, 15 from whooping-cough, 1 from ill-defined fever, 3 from enteric fever, 2 from diarrhoea, &c.

In the week ending Saturday, February 13, the mortality in twenty-eight large English towns, including London (in which the rate was 24·9), was equal to an average annual death-rate of 23·1 per 1,000 persons living; in Glasgow the rate was 29·7; and in Edinburgh 18·6. The average annual death-rate in the sixteen principal town districts of Ireland was 31·6 per 1,000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 2·9 per 1,000, the rates varying from 0·0 in Galway, Kilkenny, Drogheda, Wexford, Dundalk, Lisburn, and Armagh, to 10·3 in Lurgan; the 8 deaths from all causes registered in the last-named district comprising 2 from scarlatina. Among the 112 deaths from all causes registered in Belfast are 1 from scarlatina, 1 from whooping-cough, 1 from diphtheria, 2 from enteric fever, and 1 from diarrhoea; among the 38 deaths in Cork are 2 from typhus, 1 from diphtheria, and 2 from diarrhoea; and the 21 deaths in Londonderry comprise 1 from each of the following diseases:—Whooping-cough, enteric fever, and diarrhoea.

In the Dublin Registration District the births registered during the week amounted to 203—104 boys and 99 girls, and the deaths to 261—108 males and 153 females.

The deaths represent an annual rate of mortality of 38·5 in every 1,000 of the estimated population; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 38·0 per 1,000.

Thirty-one deaths from zymotic diseases were registered, being equal to the average for the corresponding week of the last ten years, and 3 over the number for the week ended February 6th; they comprise 1 from scarlet fever (scarlatina), 8 from typhus, 10 from whooping-cough, 1 from ill-defined fever, 5 from enteric fever, 2 from erysipelas, &c.

In the week ending Saturday, February 20, the mortality in twenty-eight large English towns, including London (in which the rate was 25·6), was equal to an average annual death-rate of 24·4 per 1,000 persons living; in Glasgow the rate was 24·8; and in Edinburgh 17·4. The average annual death-rate in the sixteen principal town districts of Ireland was 28·2 per 1,000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 2·3 per 1,000, the rates varying from 0·0 in nine of the districts to 10·3 in Lurgan; the 9 deaths from all causes registered in that district comprising 1 from measles and 1 from typhus. The 112 deaths from all causes registered in Belfast comprise 1 from measles, 1 from scarlatina, 1 from typhus, 3 from whooping-cough, and 2 from simple continued and ill-defined fever; among the 14 deaths in Waterford are 2 from diarrhoea; and the 4 deaths in Dundalk comprise 2 from whooping-cough.

In the Dublin Registration District the births registered during the week amounted to 211—119 boys and 92 girls, and the deaths to 215—104 males and 111 females.

The deaths represent an annual rate of mortality of 31·8 in every 1,000 of the estimated population; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 31·6 per 1,000.

Twenty-two deaths from zymotic diseases were registered, being 12 below the average for the corresponding week of the last ten years, and 9 under the number for the week ended February 13th; they consist of 16 from whooping-cough, and 1 from each of the following diseases:—Measles, scarlet fever (scarlatina), typhus, enteric fever, ague, and erysipelas.

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In the week ending Saturday, February 27, the mortality in twenty-eight large English towns, including London (in which the rate was 25·0), was equal to an average annual death-rate of 24·2 per 1,000 persons living. In Glasgow the rate was 31·0; and in Edinburgh 19·6. The average annual death-rate in the sixteen principal town districts of Ireland was 27·0 per 1,000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 2·5 per 1,000, the rates varying from 0·0 in nine of the districts, to 7·1 in Londonderry; the 17 deaths from all causes registered in that district comprising 1 from each of the following diseases:—Whooping-cough, simple continued fever, enteric fever, and diarrhoea. The 112 deaths from all causes registered in Belfast comprise 1 from measles, 2 from scarlatina, 2 from typhus, 3 from whooping-cough, and 1 from diphtheria. One of the two deaths registered in Sligo was caused by typhus.

In the Dublin Registration District the births registered during the week amounted to 205—115 boys and 90 girls, and the deaths to 206—99 males and 107 females.

The deaths represent an annual rate of mortality of 30·4 in every 1,000 of the estimated population; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 29·8 per 1,000.

Twenty-four deaths from zymotic diseases were registered, being 2 over the number for the preceding week, but 8 under the average for the eighth week of the last ten years; they consist of 18 from whooping-cough, 1 from diphtheria, 2 from cerebro-spinal fever, 2 from simple continued and ill-defined fever, and 1 from diarrhoea.

#### METEOROLOGY.

*Abstract of Observations made in the City of Dublin, Lat. 53° 20' N., Long. 6° 15' W., for the Month of February, 1886.*

Mean Height of Barometer,	-	-	30·096 inches.
Maximal Height of Barometer (on 8th, at 9 p.m.)	-	-	30·577 „
Minimal Height of Barometer (on 1st, at 9 a.m.),	-	-	29·319 „
Mean Dry-bulb Temperature,	-	-	39·1°.
Mean Wet-bulb Temperature,	-	-	37·4°.
Mean Dew-point Temperature,	-	-	34·9°.
Mean Elastic Force (Tension) of Aqueous Vapour,	-	-	·206 inch.
Mean Humidity,	-	-	85·6 per cent.
Highest Temperature in Shade (on 8th),	-	-	53·4°.
Lowest Temperature in Shade (on 5th),	-	-	27·3°.
Lowest Temperature on Grass (Radiation) (on 5th),	-	-	23·2°.
Mean Amount of Cloud,	-	-	72·5 per cent.
Rainfall (on 15 days),	-	-	1·981 inches.
Greatest Daily Rainfall (on 28th),	-	-	·522 inch.
General Directions of Wind,	-	-	S.E., S.W., E.

#### Remarks.

Like January, this was a cold month; but unlike January, the cold was connected with high pressure systems, and was accompanied by calms, or easterly to southerly winds. In the twenty years, 1865–84 inclusive, only once was February colder than in the present year—namely, in 1873, when the mean temperature was 37·9° compared with 39·7°—or, by Kaemtz's formula, 37·1° compared with 38·9°. The distribution of the cold in the British Islands was that usually observed in winter Continental anticyclones—the weather was most severe in the midland counties, S. and S.E. of England; less severe in Scotland, and least severe in Ireland. This was specially so from the 7th to the 12th inclusive, when fine, mild weather was experienced in Ireland and

Scotland, while hard frost and dense fogs prevailed in and about London, and in the S. and E. of England generally.

The mean height of the barometer was 30.096 inches, or 0.234 inch above the average value for February—namely, 29.862 inches. The mercury rose to 30.577 inches at 9 p.m. of the 8th, having been as low as 29.319 inches at 9 a.m. of the 1st. The observed range of atmospherical pressure was, therefore, 1.258 inches—slightly more than an inch and a quarter. The mean temperature deduced from daily readings of the dry bulb thermometer at 9 a.m. and 9 p.m. was 39.1°, or 1.5° above the value for January, 1886; that calculated by Kaemtz's formula—viz.,  $\text{min.} + (\text{max.} - \text{min.} \times .41) = \text{Mean Temp.}$ —from the means of the daily maxima and minima was 38.9°, or 3.6° below the average mean temperature for February, calculated in the same way, in the twenty years, 1865–84, inclusive (42.5°). The arithmetical mean of the maximal and minimal readings was 39.7°, compared with a twenty years' average of 43.2°. On the 8th the thermometer in the screen rose to 53.4°—wind W.S.W.; on the 5th the temperature fell to 27.3°—wind calm. The minimum on the grass was 23.2° on the same date. The diurnal range of temperature was small on many occasions. The rainfall was 1.981 inches, distributed over 15 days. The average rainfall for February in the twenty years, 1865–84, inclusive, was 2.244 inches, and the average number of rainy days was 17.6. The rainfall, therefore, and the rainy days were both decidedly below the average.

Sleet or snow fell on the 14th, 27th, and 28th. Hail was noted on the 1st, 25th, 27th, and 28th. The air was more or less foggy on the 4th and four following days, the 10th and two following days, the 15th, 19th, and three following days, and the 26th. Lunar halos were observed on the nights of the 11th and 12th, and solar halos appeared on the 12th, 13th, and 15th. High winds prevailed on five days.

At the beginning of the month, atmospherical pressure was low, under the influence of an extensive depression, the centre of which (28.60 inches) lay over the extreme south of Norway. Fresh W. to N.W. winds were blowing, with cold, showery weather. A rapid rise of the barometer took place in the rear of this depression, and on the 4th an anticyclone appeared over Ireland, subsequently moving southwards to France. This was succeeded by the development of two shallow depressions—one of which travelled in a northerly direction, while the other followed in the wake of the high pressure system to France.

The weather in the week ending Saturday, the 13th, was changeable and showery on the extreme western coasts, mild in the east of Ireland and in most parts of Scotland, but chiefly dry, cold, and foggy in the greater part of England. From the 7th to the 10th an anticyclone extended from Germany across the south of England, causing light variable winds, calms and very cold, foggy weather in that region;

while conditions were cyclonic in Ireland and Scotland, where the weather was open and changeable. On the night of the 9th temperature fell to 19° at Cambridge, 22° in London, and 23° at Hurst Castle and Oxford.

After Sunday, the 14th, a large anticyclone—in which readings at one time exceeded 30·90 inches—was formed over Russia and Northern Europe, from which a well-marked “ridge” extended south-westwards across the North Sea and the United Kingdom. Easterly winds consequently set in over the southern parts of our area, while variable airs prevailed in the north. The weather became gloomy, cold, and foggy over England, but was at times fine and bright in Ireland and Scotland. Showers of sleet fell on the N.E. coast of England, but elsewhere the weather was, as a rule, dry.

During the last week similar conditions held, so that easterly (N.E. to S.E.) winds were very prevalent, with a continuance of cold weather, much cloud and fog. Snow or sleet fell daily in the east of Scotland, north-east of England, and at the close of the period in Ireland also. On the 21st and 22nd large quantities of rain fell in Munster, in connection with shallow depressions which developed themselves within the limits of the anticyclone. On Sunday, the 28th, a cyclone approached St. George's Channel from S.W., the S.E. wind freshened to a gale at night, and a heavy snowstorm occurred in the east of Ireland, over the Irish Sea, and in Wales, subsequently extending over almost the entire kingdom.

## PERISCOPE.

### ATROPHY OF THE SUPRARENAL CAPSULES, WITH ENLARGED SPLEEN AND DARK-COLOURED URINE.

IN the “Transactions of the Pathological Society of London” for 1885, Vol. XXXVI., p. 440, Dr. Robert Saundby reports a case of atrophy of the suprarenal capsules, with enlarged spleen and dark-coloured urine. The suprarenal capsules were very small, the right weighing 18 grains and the left 10 grains. Under the microscope the capsules showed no trace of inflammation or fatty or fibroid degeneration; *they seemed to be simply atrophied*. There was no inflammatory change, the structure of the capsules being quite normal, so far as there was any. The spleen occupied the whole of the left hypochondrium; it weighed 66 ozs.; it was dark purple in colour, hard and tense. On section it looked like damson cheese. It gave no amyloid reaction. Under the microscope a large quantity of granular brown pigment was diffused through the pulp, but there was no other structural change. The urine was examined by Dr. MacMunn, of Wolverhampton, in 1882 (the patient died 22nd January, 1885), who reported as follows:—“Urine deep yellow or

brownish yellow colour, with mucous cloud at bottom of bottle. Reaction acid, sp. gr. 1012, no albumen; no blue colour with guaiacum and ozonic ether; with nitric acid gives only red and violet. The colour is due to urobilin, indican, and it also yielded onucleolin (so-called). It contained no blood-pigment and no bile-pigment." The patient was a young lady whom Dr. Saundby saw early in 1882 for some trifling indisposition, on which occasion he made the following note:—"She presents no abnormal physical signs, especially no evidence of splenic enlargement; her complexion is slightly ochre-tinged; her urine is always dark. She is now sixteen, and menstruated for a short time regularly, but this has stopped for the last six months. She appears, in other respects, a well-grown, well-developed, and intelligent girl. Her urine was dark amber, turbid, 1015, acid, no albumen; turbidity disappeared on heating; it gave a faint blue colour with guaiacum and ozonic ether." A few months later in the same year he saw her again; she was then complaining of weakness, loss of appetite, superficial ulceration of the gums, and constipation followed by diarrhoea. Her urine was darker than usual, and at this time the spleen could be readily detected by palpation. After this time (1882) she was never strong. She became more decidedly anæmic; there was a systolic murmur over the base of the heart; menstruation was irregular; she had very little appetite; her digestion was feeble, and she appeared weak and languid. In August, 1884, Dr. Saundby made the following notes:—"26th.—She came, looking very ill. Has had some annoyance. Sick; no appetite; ordered to bed. 27th.—Systolic murmur over base of heart. Spleen enlarged (a sketch in margin shows that it came nearly to the umbilicus in the middle line, and passed still lower in the left lumbar region)." In September the spleen had receded so as only to be felt just below the ribs. Her health continued to be very unsatisfactory. There was no increase noticeable in the colour of her skin, which remained much the same. Her blood was examined more than once by Dr. Saundby; there was never any increase of white corpuscles or marked diminution of the red, but the latter were pale. On January 17th, 1885, she got over-fatigued, and felt faint; and on the following day (Sunday) she was worse, and fainted, with loss of consciousness. On Monday she was sick, and complained of headache and faintness. On Tuesday temperature was 102°, pulse 120 or more, full and fairly firm. Tongue pale and clean. She complained of headache, pain at the epigastrium, and nausea. Bowels open. Spleen reached down to the umbilicus. Urine dark. She slept from 9 p.m. to midnight, and afterwards became very restless. Next day at 9 30 a.m., pulse very rapid and feeble. She could answer questions, and was free from pain. In spite of all that could be done she never rallied, and became unconscious about 5 p.m., dying comatose at 6 30 the same evening. Dr. Saundby regards the atrophy of the supra-



renal capsules in this instance as primary or independent of any previous inflammatory change, and holds that there is good reason to believe that any destructive lesion of the capsules is capable of giving rise to pigmentation. The recent observations of Dr. MacMunn have tended to show that the capsules are glands concerned in the elimination of effete blood-pigment, and his researches, if followed up and confirmed, will explain, perhaps, the most striking of the phenomena of Addison's disease. Dr. Saundby is disposed to regard the fatal result of the disease as connected with the effect of some poison in the blood, possibly a product of effete pigment.

A. W. F.

#### STANNOUS CHLORIDE AS A DISINFECTANT.

IN view of the objections to the use of mercuric chloride as a disinfectant and antiseptic, in consequence of its being a dangerous poison, and also from its exerting a very detrimental action upon lead pipes when brought in contact with them, Dr. Abbott, acting on the suggestion of Dr. T. R. Duggan, experimented upon stannous chloride ( $\text{SnCl}_2$ ), with a view of determining its influence upon living organisms. Its lack of poisonous properties, its non-corroding action on lead pipes, and the cheapness of its preparation, recommended the stannous chloride for trial, but it was found that the salt in its germicidation came far short of mercuric chloride. Nevertheless, it was ascertained that it possessed a higher potency in this capacity than many other substances commonly employed for the same purpose—*e.g.*, zinc chloride, copper sulphate, zinc sulphate, and ferric salts. In making a solution of stannous chloride intended for permanent preservation, it is advisable to add an equal weight of ammonium chloride; this prevents the formation of insoluble oxychloride of tin which would otherwise be deposited on standing.—*N. Y. Med. News*, Jan. 30.

#### PULMONARY EMBOLISM WITH RECOVERY.

KENEZY, of Buda-Pesth, reports (*Centralblatt für Gynäkologie*, 46, 1884) the following apparently well-established and very interesting case in a lying-in woman:—B. A., aged twenty-seven, confined of twins.—The patient had first menstruated at the age of seventeen, and was irregular. First labour normal. Three hours afterwards a second delivery, which also proceeded naturally. The patient, on being moved into a clean bed, was seized with great difficulty of breathing, a feeling of oppression, and—after the lapse of some minutes—a violent attack of smothering. At the same time her face and lips became livid, a few streaks of blood showed in the expectoration, and there were pains in the chest, retching, coldness of the extremities. The attack passed off quickly, but she remained rather distressed and cyanotic. Pulse, 80; temperature,  $99.7^\circ$ . The

diagnosis was settled to be pulmonary embolism. Ordered: absolute rest, hypodermic injection of morphine, sinapisms. In the evening a similar attack, accompanied with great restlessness, occurred. Next day there was pronounced œdema of the lungs. Pulse, 88; temperature, 99·1°. Ether was administered subcutaneously; ipecacuan and solution of ammonium with anise (*liquor ammonii anisatus*, *Preussische Pharmacopœe* and *Pharm. Hamb.*) were given internally. On the 3rd day the œdema was increasing. Pulse, 132. The patient's condition remained unaltered until the 5th day. There was some relief after the ether injection. Pulse, 124 to 140; temperature, 98·2° to 99·0°. During the 7th, 8th, and 10th days, the symptoms of œdema subsided. On the 14th day, mitral insufficiency and cardiac hypertrophy were recognised. From this time the patient improved continuously, and was discharged, cured, after an illness of thirty days. It might naturally be supposed that the embolus took its origin from the heart; but the entire onset and course of the illness shows that it arose from the uterus as its starting-point.—*Norsk Mag. for Lægevidenskaben*, Marts, 1886.

#### HYPODERMIC SOLUTION OF QUININE.

WHERE it is necessary to administer quinine subcutaneously, the following formula is recommended by Dr. S. S. Burt as being as little irritating as possible:—*R.* Quininæ bisulphatis,  $\text{ʒi.}$ ; acidi borici, gr. 2; morphinæ sulphatis, gr.  $\frac{1}{4}$ ; aquæ distillata,  $\text{ʒi.}$  Sig.: For hypodermic use. One drachm contains seven and a half grains of quinine.

#### CHLOROFORM WATER AS A HÆMOSTATIC.

SPAAR, of Brussels (*Med. Chir. Centr.*, Jan. 1, 1886), recommends the use of a two per cent. aqueous solution of chloroform as a hæmostatic, especially in operations in oral surgery. Its action is very rapid and simple. Washing out of the mouth is said to be sufficient to arrest bleeding, even of the large vessels.—*Med. News*, Jan. 30.

### NEW PREPARATIONS AND SCIENTIFIC INVENTIONS.

#### *Pure Terebene.*

WE have received from the well-known firm of Burroughs, Wellcome, & Co., Snow Hill Buildings, London, E.C., samples of the pure terebene manufactured by them. It is beautifully clear, and seems to be perfectly pure and of excellent quality. Although terebene is not well borne by some stomachs, and is not suitable in many cases, yet it often acts like magic in bronchorrhœa, the tympanites of adynamic fevers, and other morbid conditions in which turpentine is indicated. It may also be used as an inhalation, floating on warm water. Internally, it is most easily

taken in 5 to 10 or 15-drop doses on a lump of sugar. The profession is indebted to Dr. Murrell for drawing attention to pure terebene as a remedy for internal use, and the preparation supplied by Messrs. Burroughs, Wellcome, & Co., is deserving of all commendation.

*Meat Peptone and Peptonising Pellets.*

Messrs. Savory and Moore, Chemists to the Queen, 143 New Bond-street, London, W., have submitted to us samples of their meat peptone and peptonising pellets.

The meat peptone possesses a full meaty and not unpleasant flavour, and makes, without any addition, a palatable soup. It is stated that it contains—besides the extractives—all the fibrin and albumen of flesh, so that it thoroughly represents meat, for which it is an adequate substitute. Spread between bread and butter and sprinkled with salt, it makes an agreeable sandwich. A teaspoonful dissolved in a teacupful of hot water forms a liquid similar in taste to beef-tea. It may be administered as an enema; or, if flavoured with suitable condiments, it may be taken as soup.

The peptonising pellets are as convenient as they are active in the pre-digestion of milk. The following "Directions for Use" accompany each bottle or packet of the pellets:—

"Mix a pint of milk with a quarter of a pint of water. Divide the mixture into two equal parts. Boil the one half and add it to the cold half, which will give a mixture of exactly the temperature required, about 135° F. Or, the mixture of milk and water may, of course, be heated directly, until it is hot enough. Care must be taken not to exceed 135° F., but a few degrees of heat less are of little moment.

"After the milk is brought to the required temperature, add one pellet. Keep under a cosy or in a warm place for a quarter of an hour, occasionally stirring, when the milk will be sufficiently peptonised for ordinary use.

"If required to be fully peptonised, the milk may be allowed to stand for half an hour or more, or, if time be an object, two or three pellets may be added, and the peptonising continued until a slightly bitter flavour is detected, which indicates the completion of the process.

"The pellets can be used whole as they soon dissolve, but preferably they should be crushed with the bowl of a teaspoon or other convenient object.

"If the milk, when ready, is not taken at once, it must be raised to the boiling point for one minute to prevent the development of the bitter flavour. The milk will then keep as long or longer than ordinary milk."

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OF

## MEDICAL SCIENCE.

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## PART I. ORIGINAL COMMUNICATIONS.

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ART. XIX.—*On Bone-drainage in Hip-joint Disease.\** By W. THORNLEY STOKER, Surgeon to the Richmond Hospital.

THE nomenclature of chronic hip-joint disease has been so various and so inexact, and some of the attempts to divide the affection into stages so far removed from correspondence with clinical experience, that I find it necessary to preface any remarks I have to make on the subject of treatment, by a notice of the errors which have been made in these respects, and the corrections of them which recent surgery exhibits. It is not possible to lay down a rule of practice without defining the exact condition to which that rule applies. How inexact the old division of coxalgia into stages, as evidenced by alterations in the length of the limb, is, must have been a matter of observation to every surgeon who has watched the disease, and found that the apparent lengthening, which was supposed to constitute the leading character of the so-called first stage, nearly as often follows the apparent shortening which marks what is sometimes called the second stage, as it precedes it. In point of fact, there is no real line, either clinical, anatomical, or pathological, to be drawn between those stages; while the third, or that in which real shortening occurs, although in every sense a separate condition, owes the deformity from which it received its name to one of a number of various causes

\* Read before the Surgical Section of the Academy of Medicine in Ireland, March 19, 1886.

which the nomenclature jumbles together without any attempt at classification, although their pathology is as different as their treatment should be. A division like this, based on symptoms, and not upon the changes of which symptoms are but the expression, could have no value even if correct, and has no correctness independent of its want of value.

So many writers have divided the periods of hip disease in so many ways that it is impossible to reconcile them on paper, but every practical surgeon knows that whatever divisions he finds in books, at the bedside he arranges his cases into two classes, which correspond with sufficient accuracy to those described by Ford in his book, which was the first expression of clear views about this disease :—

First, those in which the joint structures are in a state of inflammatory or other diseased change not yet amounting to destruction, and in which apparent alteration of length either in the direction of lengthening or shortening occurs ; and

Second, those in which disintegration, more or less complete, has taken place, spoiled the articulation, and caused real shortening.

With the second class, embracing necrosis and abscess, I have no concern at present, as my remarks apply to the treatment of those less-developed instances of disease where the integrity of the articulation is not yet hopelessly impaired, where the inflammatory changes in the bones or soft tissues are susceptible of curative treatment, and where more or less complete recovery may take place.

Putting aside, as a matter for separate consideration, the important question as to whether particular cases are simple, strumous, or tubercular in their kind, the classification of most help to the practical surgeon is that which, based on the situation in which the disease has originated, divides it into *Femoral*, *Acetabular*, and *Arthritic*.

With regard to the arthritic form, recent pathological investigation seems to prove that joint diseases never occur primarily in cartilage or ligaments, that the engagements of these tissues is secondary to that of the bone or synovial membrane, that even the ligamentum teres—which from the vascular supply it carries has been so often stated to be the starting-point of hip disease—is never primarily engaged, and that the frequency with which it is found disorganised is due to osteitis of the epiphysis, with which it has so vascular a connection. Accepting as a fact that morbus

coxæ, like other joint diseases, has its commencement in either bone or synovial membrane, we have to consider in which it most commonly begins, and why it does so. Here I follow the authority of Mr. Barwell, who, giving it that his observation proves the infrequency with which the synovial membrane is primarily engaged as compared with the bones, points to the protected position of the synovial membrane of the hip as guarding it from the effects of cold and injury, and shows that from the anatomical conformation and mode of development of the bones entering into this articulation, they are rendered unusually susceptible of disease. The epiphysis both of the acetabulum and that of the femur, together with the neck of the latter bone, lie within the synovial sac; and, unlike the bones entering into the formation of other large joints, are the seat in early life of great intra-articular vascular activity, and of a consequent liability to inflammatory changes involving the joint—in obedience to the pathological law that morbid changes tend to become extended in proportion to the vascularity of the soil in which they take place. If it be asked why an opposite order of affairs obtains in the knee, where we know that chronic disease usually has its development in the synovial membrane, and spreads to the bones, it is answered by the exposed position of the membrane in the knee as contrasted with the hip, and by the exclusion from the former joint of all epiphyses, except the anterior portion of that of the femur. Of the two bones, in either of which osteal coxalgia may have its commencement, clinical observation shows that the femur is by far the most commonly affected, partly because of its more active vascularity, and partly from its greater exposure. My own observation of hip disease—which has been sufficiently extensive in a city where it is extremely common—jumps entirely with that of Mr. Barwell, who expresses himself that “there is no doubt whatever that nearly all cases of chronic infantile hip-joint disease originate in the bone, and more especially about the pelvic or epiphysal junctions.”

An interesting paper in this connection, which argues not only for the femoral origin of the disease, but for its strumous character, is that published in the *Lancet* of December 24th, 1881, by Mr. J. Greig Smith, in which he classifies the so-called white swellings of joints into those commencing in the synovial membrane, which he names “synovio-arthritis,” and those taking origin in the pink marrow in the ends of the bones, which he christens “medullo-arthritis.” In relation to the latter he points to the connection



between the pink marrow of the cancellous tissue and the lymph-glandular class of organs, and to its kindred liability in connection with these organs to strumous disease. He reminds us of the liability of abscesses formed as a result of this disease, when occurring in the ends of the large bones, to burst into the neighbouring articulation; and he details two cases in which abscesses having so involved the knee, he evacuated them by boring into the femur where originally diseased, and had excellent results. He speaks of the frequency of medullo-arthritis of the upper end of the femur as a common starting-point of hip-joint disease, and details his method of boring into the trochanter and neck of the bone for its relief—an operation to which I will presently refer. His paper is a plea for the tunnelling of diseased bone in the vicinity of joints, and the free scooping away of diseased cancellous tissue, so as not merely to make a drain for the subsequent relief of the disordered structure, but to actually remove it as far as possible at the time of operation.

I have said so much of the pathology of *morbus coxæ*, in order to lead to the conclusion I hold in common with the authors I have named, and many others, that this complaint is usually osteal in its origin, and seated at first in the femur—as upon that postulate is founded the treatment proposed by Dr. Frederick Kirkpatrick, which is essentially the same as that which I advocate. I deal with only one point of treatment, and applied to only one class of cases. The mode of treatment is that by boring into the neck of the femur through the great trochanter, so as to obtain bone-drainage, and the case to which it is applicable, that of osteitis, or medullo-arthritis—as you please to regard it—of the femur, in the vicinity of the hip-joint. I by no means wish to underrate other methods of treatment, or to set them aside, but their consideration is outside the scope of my present communication; and while rest, counter-irritation, and so forth, may have their use as special means in special cases; and while I esteem rest, for instance, as an almost necessary aid to the very procedure I now speak of, I desire to assert my strong belief in the value of a treatment which, by affording a drain to an inflamed tissue, and giving vent to diseased products, does for early hip disease what surgeons have long done for kindred conditions in other situations and in other tissues.

If it be conceded that in certain given cases hip disease has its starting point in the femoral portion of the joint, and that an inflammatory condition constitutes this commencement, it can

hardly be denied that an opening made into the diseased structures without involving the cavity of the joint is a rational and logical treatment. It only requires that it shall be possible to recognise commencing femoral coxalgia, and that the surgeon shall find it possible to select cases where the disease is mainly confined to the femur, and is in such a modified condition as to afford hope of a good result from the treatment. The cases, of course, affording the best chances of good result are those which are neither strumous nor tubercular; but all we have learned in late years of these cachectic states points to the propriety of removal of the diseased tissue and free drainage in them, no less than in the simple condition. That osteal cases can be distinguished from arthritic, and femoral from acetabular, is sufficiently evidenced by the examination of any good text-book, and, as for the condition of disease in the particular part, the selection of cases fit for operation is a matter of clinical experience more than of written rule. I am strongly of opinion that even where the disease has engaged the soft structures to a limited extent, the operation of tunnelling the bone may effect a cure, as the relief of the bone itself by the exit given to inflammatory matters removes a cause of irritation from the synovial membrane, and must have a beneficial effect. I think, therefore, that while the typical cases for this treatment are those in which the disease has only shown itself in the femur, it is also applicable to instances in which synovitis exists. Further, the plan of tunnelling may be used in the late stage of the disease, as advised by Mr. Greig Smith, as a means of draining the joint of pus or particles of necrosed bone.

The logical excellence of this treatment, and the results claimed by Dr. Kirkpatrick for his method, which is essentially the same, induced me to give it a trial, with the good effect I will presently detail.

When the theory of the treatment and the success claimed for it is considered, it is a matter of surprise that it has not been more employed, and yet we may search the text-books dealing with joint disease and not even find it mentioned. To Dr. Kirkpatrick belongs the merit of having, at the meeting of the British Medical Association in 1867, directed attention to this plan of treatment in diseases of joints—the hip among others. He described the mode of cutting down on the trochanter major, and tunnelling into it with a trephine or drill. But he adds the use of caustic potash, which he applies freely in the wound, and from the use of which I dissent.

I cannot see much harm in the use of the caustic, but I certainly see no good, and as excessive surgery is not the best surgery, I am opposed to its employment. I prefer the more precise practice of the use of instruments, and object to the caustic as not only unnecessary, but uncertain and disobedient. Putting this aside, any credit which attaches to the introduction of the treatment unquestionably belongs to Dr. Kirkpatrick. Mr. J. Greig Smith, writing on the subject fourteen years later, describes a procedure essentially the same, omitting the caustic and employing a Volkmann's spoon or scoop to remove diseased cancellous tissue from the interior of the affected bone.

I have applied the treatment of bone-drainage by tunnelling the trochanter and neck to two cases of hip-joint disease. One of them, in a child aged five, had commenced as femoral disease, and was operated on when the soft tissues of the joint had become considerably engaged, but before any pus had formed. It has improved to a marked degree, in no point more evidently than in the relief from pain which the child has experienced. I do not make more detailed mention of the case as it is still under treatment.

The second case is that of a girl named A. S., aged eleven years, who was admitted into the Richmond Hospital in July, 1885, at the request of my friend, Mr. S. T. Reede, of Carrickmacross, suffering from disease of the right hip-joint of four months' standing. She limped and walked with difficulty, had starting pain in the hip, and pain at the inner condyle. The limb was apparently shortened, and the foot everted. No swelling existed behind the trochanter, or in the groin. The trochanter was slightly enlarged, and tender when pressed on. She was in the late part of the first of those two stages into which, as I have said, the best clinical experience divides the complaint. As her general health was suffering, her lameness and deformity increasing, as rest had failed to give relief, and as it was a purely femoral case of disease, I trephined the trochanter on July 8th. No additional constitutional disturbance showed itself. She was kept in bed until July 23, when her pain had so far abated that she could walk without inconvenience, and left hospital to return to her country home. At the time of her discharge the sinus continued to discharge, but in very small quantity. The discharge lasted for about three weeks, during which time she was kept chiefly in the recumbent position on a sofa. At the end of this period she was gradually allowed to resume her former active habits,

and I saw her on December 7th in a perfectly sound condition, able to run and jump with any girl of her age—the only evidence that she had ever been in the hands of a surgeon being a dimple over the lower part of her trochanter.

When we think of the sad progress of most of the cases of this kind, and of the great length of time occupied by other treatments, even when successful, we ought at least to give the gravest consideration to the propriety of a measure which, like this, is capable of effecting a cure in a little over a month.

The method of operation I follow is to make a vertical incision down to the trochanter, with its centre over the point at which the bone is to be penetrated; to remove the bone to the full depth of a trephine of half inch diameter, and if additional tunnelling towards the head be deemed necessary to perform it with a drill or strong director. I then fill the wound with strips of gauze to check immediate bleeding. The gauze is removed next day, and the wound henceforward kept covered with a moist carbolic dressing. As soon as it has effected its purpose of drainage, the sinus closes without further treatment.

Mr. Greig Smith has laid down a very good rule as to the point at which to enter the trochanter, so as to get safely into the neck. Three prominent points can be felt around the great trochanter. These are the anterior superior, anterior inferior, and posterior inferior angles, and they nearly form the angles of an equilateral triangle. The junction of the anterior and middle thirds of the base line is the proper place to perforate the bone. Comparison of the femur at different ages will show that in entering the bone from this point it is necessary, in order to preserve the integrity of the compact tissue covering the neck, to give the tunnel a more oblique direction upwards in young objects, and approaching the horizontal line in older ones, because of the different angle assumed by the neck as age advances.

Inspection from above shows that any penetration at a point *posterior* to that laid down would involve a risk of opening through the back of the neck of the bone.

Founded on what I have said, I put forward these propositions:—

1st. That tunnelling the trochanter and neck is a reasonable and good practice in cases of femoral coxalgia, and is calculated to afford drainage and remove the products of disease.

2nd. That it is in better accord with surgical science, while adopting the essential principle of Dr. Kirkpatrick's plan, to avoid

the employment of caustic and rely upon the more precise use of instruments; and

3rd. That the extended application of the plan, so as to remove the diseased bone or even to drain a joint containing pus, as proposed by Mr. G. Smith, is a surgical proceeding worthy of every examination.

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ART. XX.—*Select Clinical Reports on Cases of*—(I.) *Generalised Locomotor Ataxy*; (II.) *Progressive Bulbar (Labio-Glossolaryngeal) Paralysis*; (III.) *Progressive Muscular Atrophy, complicated with Bulbar Paralysis*. By ARTHUR WYNNE FOOT, M.D. Univ. Dubl.; Senior Physician to the Meath Hospital; Professor of Medicine in the Royal College of Surgeons in Ireland.

### *I. Generalised Locomotor Ataxy.*

THE patient in the following case was under my care in the Meath Hospital, and was afterwards under observation or in frequent correspondence with me for a period of ten years.

It was one of generalised locomotor ataxy—that is, of ataxia affecting both the upper and lower extremities.

I have endeavoured by due curtailment and by omission of unnecessary detail to avoid prolixity in the record of a case extending over such a length of time.

Cases of this disease are generally progressive—from bad to worse, but this one seemed to progress from worse to better. The patient assisted my observations in every way, and co-operated with (as might be supposed) a lively personal interest in carrying out the treatment. In brief, it may be stated that after having had to abandon the practice of his profession and come into a general hospital, he was in time enabled to return to work, and was doing well thereat when he succumbed to an attack of bronchitis.

As he is now more than four years dead, I have no hesitation in complying with a wish he often expressed—that I would write out and make known his case.

It may be observed that the patient had the advantage of the best medical opinions obtainable either in America or in England.

Dr. X. had a considerable practice as colonial surgeon in Vancouver Island, which he was obliged to give up because, from paræsthesia of the hands, he became unable to feel either the

presentations or the positions in midwifery. He could not even be certain about the conditions of the os uteri, so much was his sense of touch impaired. He had the best advice procurable in America; took a large amount of nitrate of silver; went through three courses of bichloride of mercury, the last of which he thought induced anthrax; used a quantity of Parrish's syrup; had strychnine both plain and combined with iron; and went through a course of hydropathy, with shower and other kinds of salt-water baths for six months. The last method of treatment he considered did him more good than any other. Coming over to Ireland in the spring of 1871 he expressed a wish to put himself under treatment in the Meath Hospital, and was admitted into my wards on May 16th, 1871.

At that time Dr X. was thirty-eight years of age, unmarried; height, 5 feet 8½ inches; weight, 164 lbs. (11 stone 10 lbs.), of active and temperate habits, and sanguineous complexion. In the year 1861 he had contracted syphilis; in the ensuing year he had papular eruption, and a mild attack of iritis; for the last nine years there had been no further external manifestations of syphilis. His mother died of bulbar paralysis; there was no other family illustration of a tendency to nervous disease. The patient was, and had been, of temperate habits; he had never been a great smoker, and had given it up completely two years before admission. He pleaded guilty to sexual excesses, indulged in in Edinburgh, in the year 1865, when, under favourable circumstances, he discovered a singular aptitude for repeating the venereal act a great many times within a short period. At this date he used to have connection six or seven times a night three times in the week, and continued to operate at this ratio for nearly a year. Dr. X. often discussed with me the question whether these libidinous excesses were the cause or the result of his disease, and we generally came to the conclusion they were more likely to have been the result. In 1871, when admitted to the Meath, the sexual excitement had entirely subsided. He had nocturnal priapism occasionally, unattended with desire, but seminal emissions were of very rare occurrence. During midsummer of the year following the sexual excess (1866) he remarked that he used to feel tired after an ordinary walk of two or three miles, whereas before that he could walk eight or ten miles without feeling an equal amount of fatigue. At such times while tired he had transient diplopia, disappearing after rest. About the same time he observed that on bending his neck quickly he was conscious of a slight constriction about the region of the first dorsal vertebra, and that at

the same moment he used to feel a sensation, like an electro-magnetic shock, passing from his back to the tips of his fingers. Weakness in the lumbar region of the spine, with a tendency to perspire there, and torpidity of the bowels include the remainder of the earlier symptoms which attracted his attention. Towards the close of the year 1866, as he became more incapable of locomotion, he gradually lost much of the proper use of the right foot, so that it "came flat to the ground with a peculiar flap, the instep being very weak." About the same time (end of 1866) sensation became impaired in the feet, legs, and arms on either side.

Within the next year (1867) he recovered much of the lost power over the right foot, while all the other symptoms were rather on the increase, and it was in this year he first became aware of a "want of confidence," and of a difficulty in maintaining his balance while moving in the dark. He could not keep from swaying to and fro when his eyes were covered washing his face in a basin of water. Early in 1868 he noticed increasing failure of sensation, and of co-ordinating power in either hand and arm, commencing in the fingers of the left hand, while about the same time he began to experience a sensation in the spinal region as if a cloth wrung out of warm water were in contact with his back. This hot feel was unattended with pain or tenderness, and was usually most observed when he was fatigued—it was seldom present in the mornings. He had at the same time a feeling of warmth over the scapular region whenever he did not feel well, or, as he used to put it, whenever he had this hot feeling about the scapulæ he had a bad day.

In 1868 some bladder symptoms were noticed, but nothing more than delay in micturition. In 1870 he became subject to slight œdema of the hands and feet, which slowly increased, also to occasional pains in the left hand and arm, and to what he called spasms at night in the calves; he had besides a feeling as if the arm were pressed down by a heavy weight laid on the shoulder, and there was the same sensation of weight about the hips also. Common sensation was much impaired over the nates and thighs. He had never suffered from the sharp-shooting, electric, or fulgurant pains common in the præataxic stage. These lancinating pains, as pointed out by Fournier, are particularly mild in the syphilitic variety of ataxy, and do not, therefore, always give a distinct warning of the approaching disease. The day after admission (May, 1871), when looked at as he attempted to walk about the ward, his gait was the embodiment of incapacity and indecision. He put his feet to the ground by the

heels in a hesitating manner, trusting almost entirely to the support of a stout stick. He was unable to stand with his eyes shut, or stir at night without a light. Though he used to be very athletic, and a good jumper, he "could not now jump an inch for £100," nor make even an attempt to run. He turned round clumsily, slowly, and with deliberate preparation. His limbs were finely developed, muscular, and shapely, and he could kick about in every direction as he lay in bed, and scratch one leg with the toe of the other. The deficiency of the muscular sense was very marked; he could not estimate the physical qualities—shape, weight, kind of surface, &c.—of an object without seeing it, miscalculated the amount of exertion required to lift a tumbler from a table, and has jerked a wine-glass up so suddenly as to break it. He was unable to tell me the difference in size or weight between a half-crown, shilling, and sovereign, smashes an egg from the unnecessary amount of force he uses to grasp it, and knocks a boiled egg to pieces in his efforts to chip it. He cannot cut a slice of bread from inability to estimate the proper degree of force required. He could not tell the difference in size between a gold hunting watch and a half-crown when they were put into his hands with his eyes shut, nor say whether the watch he held was a flat or round article till he had seen it. When I was making these experiments with him, he asked me not to put anything valuable into his hands if it was fragile, as he would be likely to break it from the force with which he held it. The gross muscular strength of his grasp was sufficiently well preserved; the mean of nine trials made on each hand at different times gave 60 lbs. as the average pressure of the right hand, and 50 lbs. of the left. He did not know the dynamometer was oval without seeing it, nor could he fix it in his hand without looking at it. He used to account for the great strength he exerted in holding an object partly by the necessity of squeezing it firmly in order to feel it, and partly from fear he would drop it if it was not held very tightly. He was able to wind his watch, but did so in a slow and clumsy manner, with his eyes intently fixed upon the key. The skin of the face is the only part of the body-surface which is not more or less anæsthetic, and he avails himself of it to light a match at night. He grasps a bundle of them in his hand, and, by applying the handful to his cheek, he after a while manages to discriminate one or two which he contrives to strike after many efforts. This process is so troublesome and uncertain that he has long kept a light burning at night. This is the more necessary for him, as in the dark he cannot tell



where his legs are. One night he felt very cold, but could not make out how he was lying, though he tried to "take soundings" with his feet. At last he called for a light, and found he was lying diagonally across the bed, and that the coverings had slipped off him. The skin is very sensitive to thermic influences, though so obtuse to tactile impressions. A cold key applied to the skin makes him start, and he often experienced quite a shock from some of the articles attached to his watch chain touching the backs of his hands if he did not see them. The knee-jerk had been long absent. Plantar reflex was slight in the left sole, absent in the right. He was conscious of touch in either foot. The sense of touch was much more defective in the left than in the right hand. The left upper and right lower extremities were the parts most affected. He cannot button any part of his dress, nor fasten anything with a pin without seeing it; has a difficulty in writing, and holds the pen stuck into his fist, jammed firmly between the thumb and the metacarpal bone of the index finger, all the fingers being meanwhile strongly flexed, so that the pen is guided entirely by wrist, not by finger movement. His habitual pulse was 65, and regular. Heart, lungs, and stomach were in good condition; he slept well, and had an excellent appetite; has been subject to tinnitus aurium on left side for last six months, especially when fatigued; is free from it in the mornings. Sight not appreciably affected, but the pupils were contracted, the eyes red and ferrety, especially the right, and there was occasional diplopia. The diplopia is illustrated by his seeing two moons when fatigued, and on walking along the street he would see a second kerb-stone at an acute angle to the real one, moving forwards as he walked, and always to the right of the real one. With eyes closed he cannot touch the tip of his nose with his finger, unless after many trials, and has no chance of doing so with a pencil held in the hand.

At this visit he remained for fourteen weeks under my care—16th May to 23rd August, 1871. During this time the treatment was mainly electrical. The instrument used was Störher's large induction battery. The current of the secondary coil was applied daily for from 5 to 15 minutes along the spine from the seventh cervical to the fourth lumbar vertebra. The electric brush was frequently used over the hands. The bowels were habitually costive, and he was invariably worse when they were allowed to become confined. They were regulated by the use of sulph. quin. gr. 1, + pulv. nuc. vom. gr. 1, + ext. al. Barb. gr. 2, + ext. rhei. gr. 2, in pil. ij; sum. diebus alternis. He was at the same time taking hypophosphite of sodium

(gr. 5 ter die) and full doses of cod-liver oil ( $\frac{1}{2}$  oz.) as often. In July, 1871, the late Dr. A. Hudson was good enough to see him, and suggested the addition of ergot of rye (gr. 5 ter die), which was adopted. He was by this time able to stay three times as long on his legs as when admitted; also able to stoop and pick up a thing off the ground much better, and to make a more correct estimate of the force required to grasp an object with. In August he was able to walk as far as the Phoenix Park by taking his time, and had acquired more confidence in going down stairs. August 14th.—He is still caught in the back when he goes to stand up after sitting a while, and still has marked diplopia when fatigued; but he is now able to chip an egg and cut a slice of bread from a loaf, which he could not do when admitted. He can now walk two miles with less fatigue than he could a quarter of a mile two months ago. He is beginning to lose the indiarubber cushiony feel he used have in the soles of his feet when they were pressed against the ground. August 20th.—His eyes were examined by Mr. Henry Wilson, who found nothing more than some hypermetropia in the left eye. He left hospital to go to the country on August 23rd, 1871. During his stay in hospital the urine was often scanty, but never contained either sugar or albumen. On one occasion he passed none for 20 hours; at another time the output of more than 48 hours was but 41 oz. On three other occasions on which the 24 hours' urine was collected for analysis the quantities were 30, 30, and 43 oz.—the urea varied from 32 to 33.50 grs. per 1,000 grs., the phosphoric acid from 1.8 to 2.90 grs. per 1,000 grs. When the kidneys were acting insufficiently a glossy tension of the integument about the roots of the finger nails was noticeable, and some puffing of the backs of the hands, attended with dull pain in the lumbar region. Under these circumstances dry cupping was applied to the loins, and gin punch was given at bed-time.

He returned to hospital early in October, 1871, especially with the object of getting some relief for the anæsthesia in his hands. While in the country in the summer he had been able to drive a car at night, and had avoided collision with other vehicles on the road. In holding the reins he used the two hands. He cannot button his dress without looking at it, and has to stand before a glass to fasten his shirt front. If he rubs or presses his finger violently against a trouser button he can feel it, but no more; the operation of getting it through a button-hole is too complicated for him. The face is the only part not more or less anæsthetic. On the back he

feels as one point two fingers spanned as widely apart as possible. On this occasion he remained in hospital for eleven weeks. The electrical treatment was resumed; the continuous current from a Smee's battery was used, generally 40 cells were employed. The current was administered daily for about ten minutes at a time, and along the spine. During its application he often expressed himself as being invigorated by its use. He frequently said he felt "braced up," or "laced in stays," or "strengthened in the back and arms." The P. pole was kept fixed in the middle cervical region, while the N. pole was moved about over the lower part of the spinal column. October 28th.—After the use of 50 cells for ten minutes he went out and walked about town, and was able to get up the staircase to the top landing of the hospital without holding the bannisters—it was the first time he had been able to do so. His internal treatment was the same as previously. He left hospital on December 22nd, 1871, much improved in locomotion, but as yet quite unable to feel his pulse.

In the early part of the summer of 1872 he attended hospital as an out-patient, and was galvanised daily along the spine, for five weeks, with a current from 40 Smee's cells. He said he was a different man as far as his legs were concerned from what he had been when he first came to the hospital. He had now none of the cottony feel in the soles, and felt the ground as well as ever, but he had no improvement to speak of in the manipulation or power of feeling in his fingers—could not feel his own pulse, or discriminate two wooden matches.

In 1873 he went through a course of the iodides of potassium and iron, and then through one of phosphorus and cod-liver oil, and felt so much better that in the summer of this year he made voyages to the River Plate, as surgeon of a steamer, at £10 a month. He surprised me by saying that he had been able to keep his feet on the deck in a gale of wind. On his return I put him on fer. arsen. gr. 1, and fer. iod. gr. 15 in pil xv.—s. 1 t. d.

In the winter of 1873–74 he was backwards and forwards to the River Plate. In March, 1874, he told me that, although he could yet make but a poor attempt to run or jump, he should have no hesitation in undertaking a general practice had he proper sensation in his hands. There was an improvement in his power of manipulation, as he had extracted some teeth successfully on his last voyage. He was still subject to transient œdema of the finger-tips when the action of the kidneys was deficient.

In the spring of the following year, 1875, he was in London,

and consulted Drs. Wilks, Radcliffe, and Herbert Tibbits. Dr. Radcliffe advised him to persevere in the use of arsenic, and Dr. H. Tibbits to have an induction current applied with the wire brush to the hands and other anæsthetic parts. He took the arsenic, as recommended by Dr. Radcliffe, for more than a year, and when I next saw him, in May, 1876, the sensation in his hands had much improved; he could feel a pulse, unless it was a very feeble one; was able to walk without a stick; and could make an attempt to run. He found that whisky made him more handy with his limbs; tobacco made him less so. In the beginning of 1879 he took an easy practice in Cheshire; and in January, 1880, wrote to me that he had been steadily improving, and had given up taking medicine; that although he was unable to run, jump, or indulge in any exercise requiring much activity, he was, for all important purposes, as well as he need be. The "power of sensation," he added, "is as near perfection as possible." After another year I heard from him that his sexual potentialities were in their normal condition. He, unfortunately, got a severe attack of bronchitis in mid-winter, and died, after three days' illness, on December 28th, 1881. Of this illness I was never able to obtain any accurate account.

Up to rather modern times this disease (locomotor ataxy), under the name of *tabes dorsalis* (as old as Hippocrates, or, at all events, of Lommius), was supposed to be due to sexual excesses. Since the researches of Fournier the question of the relation of *tabes* to syphilis, though never ignored, has come into a more forward position. In 1875 M. Fournier, of Paris, adduced evidence of syphilis in 24 out of 30 ataxics. It is remarkable how rarely the disease is observed among female prostitutes. Now, if syphilis is the chief cause of the affection, prostitutes, who are so often syphilitic, ought to be affected with locomotor ataxy much more frequently than the general mass of the female population, whereas, like all other essential diseases of the cord, it is unusually rare in women. In the case just related the venereal excesses were indulged in several years subsequent to the acquisition of syphilis, which had been contracted at a time when the satyriasis, if it may be so called, had not been awakened; so that allowing equal ætiological moment to the two influences—syphilis and sexual excess—the syphilis takes precedence by priority in point of time.

The frequency and importance of syphilis as an antecedent to locomotor ataxy is now generally recognised. It is also a remark-

able fact that this implication of the spinal cord, which is generally one of the tertiary manifestations of the poison, succeeds with much greater frequency to the milder than to the severer forms of syphilitic disease. According to Remak, both these causes—that is, sexual excess on the one hand, and syphilis on the other—are competent to induce the disease. In the former case, when from sexual excess, Remak considers the central nervous apparatuses are primarily affected and disturbed in their nutrition by over-irritation and over-secretion, and in consequence primary atrophy and degeneration of the same take place. In the latter case, when from syphilis, it is through the primary development of an inflammatory process in the interstitial tissue of the posterior columns, which only secondarily induces atrophy and degeneration of the nervous elements. Without vouching for their correctness, Erb regards these theories as the most plausible that have been advanced. He will admit a predisposing influence for syphilis. He doubts the existence of an actual, specific, syphilitic sclerosis of the posterior columns. He thinks it difficult, in view of the comparative frequency of both forms of disease—ataxia with and without syphilis—to exclude their accidental concurrence.

It is plain, from the predominance of symptoms in the upper extremities, that the spinal lesion in the above case was more marked in the cervical than in the lumbar region, and on that account it is the more surprising that the patient escaped the optic atrophy so frequently found—difficult as it may be to find an anatomical reason therefor—when the disease is situated high up in the cord.

The early, or præataxic, stage of the disease was singularly free from the pains which usually characterise it. He did not suffer from the boring, the fulgurant, or the constrictive (belt-like or girdle) pains. It was not that these pains escaped notice under the guise of rheumatism, sciatica, neuralgia, or flying gout, as they are apt to do, but he never suffered from them to any degree which impressed itself on his memory. If Fournier's opinion, that these pains are particularly mild in the syphilitic variety of the disease, prove correct, the observation will, perhaps, become an important one as indicative of treatment.

However, Buzzard remarks that, as a matter of fact, in the case of *tabes dorsalis*, expectations that may have been formed as to the result of specific treatment have not been realised. In his experience he has never known a cure to result from specific

measures. From mercurial treatment, which he has had very carefully applied by inunction, in a number of cases where the history of syphilis was distinct, he has seen no good whatever, but, on the contrary, as it seemed to him, a tendency to harm.

## II. *Progressive Bulbar (Labio-glosso-laryngeal) Paralysis.*

Bernard K., aged forty-five years, married, of temperate habits, was admitted to the Meath Hospital the first week in August, 1872. His height was 5 ft. 4 in.; weight (naked), 128 lbs. (9 st. 2 lb.)—he had formerly weighed 11 stone. He came from Maryport, in Cumberland, where he had been employed in an iron foundry. I heard that he had been treated by Dr. Hughes Bennett in Edinburgh.

When he came under my notice his disease had already made great progress; its characters were well marked, and his condition was as follows:—Paralysis of the orbicularis oris and others of the muscles of the lower half of the face; weakness or paralysis of the tongue, affecting articulation so much as to make his speech almost unintelligible and to seriously interfere with deglutition; partial paralysis of the larynx, making up that combination of disturbed function to which has been given the name of labio-glosso-laryngeal paralysis. The saliva had been continually dribbling from his mouth for the past five months; as he is unable to spit, he keeps wiping it away with pocket handkerchiefs; at night it wets his pillow as it flows away. He generally lies on his face for the purpose of facilitating its escape. There is no fœtor on his breath, and he has never taken mercury. His mouth is constantly open, and the lower half of his face is devoid of expression; he cannot purse up his lips so as to whistle or spit, nor can he expand the *alæ nasi*, nor move the *occipito-frontalis* or *corrugator supercillii* muscles. When he attempts to frown the forehead remains smooth, while the angles of the mouth are drawn down by the *platysma myoides*. The *zygomatici* are seen in action when he tries to smile, and the *orbiculares oculi* seem to maintain their tone, judging from the crow's feet at the corners of his eyes.

As to his tongue, it feels swollen, feels too large for his mouth; yet it does not appear of greater size than usual. It can be protruded, the tip turned to either side, and the point curved up; yet its movements are slow and its force is feeble. He cannot curl up the sides of the tongue so as to make a longitudinal channel of its *dorsum*. It is soft and flabby to the feel, is of a grayish colour, and has a sodden, wrinkled aspect. He frequently refers to uncomfortable sensations in connection with the base of the tongue, such as that there is a swelling at the root of it; that it (the tongue) is choking him; that "it has fallen down to the right side." The left half of the soft palate hangs at a lower level

than the right half; no part of the velum showed reflex contractions when tickled. Food, especially liquids, often "go the wrong way" in being swallowed, and are at times regurgitated through the nose. Food lodged between his cheeks and teeth from paralysis of the buccinators, but he is usually able to displace it with his tongue.

Mr. Smyly was good enough to examine his larynx for me; he found the right arytenoid muscle lying flaccid, and right vocal cord semi-paralysed. His voice was weak and monotonous; he could not call out loudly.

The principal and most constant trouble from which he suffers is what he calls "the working of the nerves," and which he explains to mean a creeping and tingling sensation all over the body, but more particularly in the lower limbs. This has annoyed him from the first, makes him restless, and keeps him from sleeping at night, as he has to get up and walk about to relieve this sensation. He frequently uses the expression, "the nerves are killing me." When the "nerves" are working, fibrillary contractions or flickering movements in the muscular fasciculi are visible, and palpable as a purring tremor, audible with the stethoscope as a susurrus of a deep burring tone. These fibrillary contractions were very obvious in the glutei, and in the muscles of the thighs and calves. They were more marked in the right arm and leg than in the opposite side. The right arm and leg were weaker than the left, and cutaneous sensibility was deficient along the outer side of the right lower limb from the great trochanter to the lower third of the leg. He is well able to walk about, turn round, and maintain his balance. There is no sign of muscular atrophy about the hands; the pupils were equal and normal and the eyeballs steady. Pulse slow, averaging 60; sounds of heart natural but rather weak, rhythm normal. No objective rise of temperature, though he has a subjective sensation of "burning heat in him," and he "is never cold." When asked has he pain anywhere, he replies, in his head, which he keeps bound round with a red handkerchief; the pain is chiefly referred to the back of the head, but is also felt in the sides and front. The week after admission his urine was carefully examined. It was free from sugar and albumen; total quantity (24 hours)=25 oz.; acid, 1021. Urea=28 grs. per 1,000 grs. Phosphoric acid, 2.6 grs. per 1,000; total quantity of urea=306.25 grs.; of phosphoric acid=28.43 grs. The debility of the tongue was better exemplified by its failure in the delicate movements necessary for articulation than in the grosser actions of protrusion, retraction, &c. Of the vowel sounds, *a* was the only one he could make distinctly. The pronunciation of the lingual consonants, *l*, *d*, *s*, *c*, *r*, as in the words *lady*, *scissors*, could not be effected, and so he could say these words only very slowly and with visible effort. There was a word which he had particular difficulty in pronouncing, because it combines the vowel and consonants which most require healthy action in the tongue to sound.

This was the word "anti-trinitarians." Anyone can satisfy himself of this by trying to pronounce it while the tongue is held down with a paper knife or the finger. He used be able to say this word easily; he smiled when he heard it, and said there were plenty of those folk about Maryport. The letter w was a great difficulty to him. His attempts to say "a blue fish wriggling on a hook" were almost ludicrous. The two points of the æsthiometer are felt at half an inch apart on the tip of the tongue—at one and seven-eighths on the dorsum. The sense of taste is unaffected. Respiratory action is very feeble, the fullest inspiration hardly produces any expansion of his chest. He is able to blow out a candle at a foot distance. He cannot sneeze nor laugh, though he smiles in a grim manner. He is not able to light his pipe because he cannot draw it, nor can he keep it alight when it is given to him kindled.

According to his wife's account this disease came on him suddenly; he came home one day reeling as if drunk, and complaining of his head. However, before he gave up his employment he had got into a peculiar nervous state, which she attributed to the dangerous nature of his occupation. He came to be easily startled at noises which he had not been accustomed to mind when well. For example, a mineral train used to pass his house, and the whistle of the engine annoyed him so much that he said he could not stand it, and must leave the place. The man himself attributed his disease to a fright he got from nearly stepping into a stream of molten iron a foot deep, owing to his having taken a weakness while standing beside the flowing metal.

Immediately after admission a galvanic current from 20 Smee's cells was passed through the neck, the P. pole in the sub-aural position, the N. pole was moved about over the larynx and sides of the neck; the K. C. C. (kathodic closure contraction) was strongly marked. Current kept for 7 min. to right, for 8 min. to left of neck. In the evening of this day he said "the nerves"—the expression which he always employed for the fibrillary tremors—were much better since he had been galvanised, and that that is what he wants in the legs and arms. He was in great distress owing to the accumulation in the mouth and pharynx of stringy, viscid saliva, which he said was choking him; he was making ineffectual attempts to vomit; a mouth-wash of alum relieved him very much by detaching the adhesive mucus. The galvanic current of 20 cells was daily applied to the neck; he could not bear more than 30 along the spine, as he was particularly sensitive about the lumbar region of the back; he did not feel 30 cells along either leg. The left leg was also cold subjectively; he "never can get it warmed," especially along its outer side. The galvanism gave him so much relief at first from the fibrillary twitchings, or "the nerves," as he called them, that he used to say, "It is a cure, God bless you," and used to take the conductors from me to press them himself to the places where he felt the nerves working



most. He himself discovered that he could mitigate the twitchings by holding both hands tightly round the popliteal space, which led to my fastening tourniquets in these situations and afterwards round each arm. These he kept on night and day so continuously and so tightly that it was necessary to see he did not obstruct the circulation too much.

At first he made a marked improvement, inasmuch as he came to speak more distinctly; there was less dribbling from his mouth, and "the nerves" did not torment him so much, so that his wife, at a visit to him a fortnight after his admission, was astonished to find him so much better. On this occasion he betrayed great emotion, crying very much, while, at the same time, he kept saying he was cured. His face had a most curious expression from a mixture of paralysis and wrinkles. He was much the worse of this interview, and in the afternoon I found him on the avenue waiting for me to give him "the shock," as he called the electricity, "to quiet the nerves." As was invariably the case when the "nerves" were bad, the salivation was very much increased. Soon after this date, the mouth began to be strongly drawn to the left side in speaking, and he began to complain of his "tongue falling back against his swallow and choking him;" this, I think, was due to paralysis of the branches of the hypoglossal nerve going to the stylo-glossus and genio-hyo-glossus muscles. He found that by pressing his finger into the right side of the neck, behind the angle of the jaw, he could speak more plainly—probably by supporting the stylo-glossus muscle. The cervical muscles on the left side responded to the induced current much more vigorously than those on the right. The motor branch of the 5th nerve on the right side became affected; he said his jaw was too weak on that side to allow him to chew with it, and bread sopped in milk stuck between his cheek and his teeth.

The attacks of "the nerves" begin in the head, and spread downwards through the limbs. When these attacks come on, he gets into a state of restless excitement, leaves his bed, and walks up and down the ward and passages; the stringy saliva dribbles from his lips, and he says he is choking, between the saliva and the tongue falling back, and that the "nerves" are killing him. He looks such a curious figure, roaming about, with all kinds of strings and straps fastened on by devices of his own contrivance, that he is generally regarded by the patients as insane, which, indeed, he eventually became. He keeps a red cotton handkerchief tightly fastened round his head, bandages tied round his wrists and knees, and a belt round his waist. He is very emotional, frequently crying, and asking will he die; otherwise he is a hard, rough, coarse, ignorant man. Nothing relieves him in these attacks but a strong dose of electricity, and that only temporarily. He keeps about the hospital gate in the evenings (midsummer, 1872) in hopes of my paying a visit that he may get "the shock." He was anxious to be allowed to have the battery left with him in action, that he might constantly use

it, but, as he pressed the conductors to all parts of his head and face, he could not be trusted with it. The constant current quieted him most; but when that battery was not available, he got great relief from a strong secondary current of Störher's larger induction instrument. The relief must have been considerable, judging from the blessings invoked upon the operator during its employment. He said on one occasion that he could "go to sleep while the current was passing through him." By degrees, from failure of the trapezius muscle, his head began to hang forwards upon his chest, so that his face could not be well seen till his head was raised up. He said the nerves were driving him mad, and that he would have jumped out of the window only for the iron bars. He often asked me for an emetic to remove the slimy mucus from his throat; for a long time I could not imagine what he was asking for so earnestly and urgently. His efforts to say the word "vomit" resulted in a sound like "hey-haught." I was afraid to give him a "vomit" lest any of the contents of the stomach should get into the larynx. While asking for the vomit he spreads the viscid saliva on his fingers to show me how it chokes him. On 2nd September, 1872, he left hospital, without leave, under the impression that he was about to be left alone in his ward. A day or two afterwards his wife came to tell me she was worried out of her life with him, and could not manage him at all. I re-admitted him on Sept. 9th. The mouth was quite pulled to the left side, the tongue protruded to the right. He was quite unable to whistle; in the effort to purse up his lips to do so he contracts his brows, otherwise he cannot frown—that is, as an unassisted effort of the corrugator supercillii muscle; can only spit in an awkward, spluttering manner; the right side of the tongue is relaxed and wrinkled; he can swallow fluids only slowly and by inclining his head backwards, otherwise they flow out of his mouth. There are at times arched wrinkles over the left eyebrow, while the skin over the right is smooth and flat. The nerves were quite as bad as on his former visit. On the 14th he said they were "eating his heart out." His temper underwent a change, and he became morose and sullenly savage. In the course of this month I injected atropia several times in the hopes of lessening the salivary secretion, beginning with  $\frac{1}{30}$ th gr. of the sulphate. On 21st September he again left the hospital, without leave. I took him in once more on October 5th, but in three days after was obliged to discharge him, advising his wife to get him into a lunatic asylum. I was never able to hear what eventually became of him.

### III. *Progressive Muscular Atrophy, complicated with Bulbar Paralysis.*

T. C., aged fifty-two, was admitted into the Meath Hospital on March 8th, 1883. He was a man of full size—weight, 178 lbs. (12 stone 10 lbs.); height, 5 ft. 9 in. He had led an active life, and

had not known what a day's sickness was. He was a steward or kind of sub-agent to a large landed proprietor in the south of Ireland. He had used alcohol often and largely, "in the way of business," but told me he had never been drunk in his life. He denied having ever had venereal, was father of ten children, all living, and there was no family history of a neurotic nature. When admitted the most obvious ailment was a clipping or slurring of his words (defective articulation), rendering it difficult to understand what he was saying. This thick manner of speaking has led to his having been thought to be intoxicated; thus an acquaintance observed to him, a short time before in Tralee, that he had been "drunk every day he had met him" for the last three months. Besides this embarrassment in articulation he has a difficulty in whistling, can spit but badly, and cannot blow out a candle unless it is very close to his face. He has also to be very slow and careful in swallowing, especially liquids, else they will get into the larynx; sometimes when this has occurred the food has been violently regurgitated through the nose and mouth. He had also a marked degree of atrophy of the first and second interosseous spaces of the right hand.

The first symptom he observed was in March, 1882, and was a loss of the grasp of the pen in writing and an unsteadiness of the hand—the hand used to make a start and a "scraub" of the pen, and pronate strongly. At this time he used to suffer from acute pain in the "calf of the thumb," removed by rest. In the course of the following July and August he suffered from severe pains fixed in the right trapezius and sternomastoid muscles running upwards to the occipital bone. The finer movements of the right hand were defective—he could not wind his watch, write, pinch, pick, nor button. There was a continual sensation of heat about the atrophied muscles of the hand, with first and second interosseous spaces of the right hand hollow and wasted. About Christmas, 1882, the oral and lingual symptoms came on. He is conscious of an unusual difficulty in getting his food through the second stage of mastication—i.e., from the mouth to the pharynx, and has to be most cautious about it; his saliva even often goes against his breath. His tongue is easily tired after the exertion of speaking. There are fibrillary contractions visible in the muscles of the left as well as the right side of body; he feels them also in his head, neck, eyes, and tongue—they are not painful. He persists in calling them "shocks" or "jumping in the flesh." When his lips are tapped lightly with a glass rod these fibrillary contractions are mechanically excited, and twitchings are seen in the course of the orbicularis oris running the length of the lip. When excited, his voice assumes a weird hollow tone, he cannot control its inflections; when laughing, the sound runs up into a "squall." He is emotional, and easily moved to laugh as well as cry. Emotional excitement increases the salivary secretion, and at such times its escape from his powerless lips is very obvious. Food

frequently lodges between his cheeks and teeth, and often falls from his mouth when eating.

In the course of the autumn of 1882 he began to fail on his legs, and since then they have been losing flesh, and becoming soft and flabby. He can walk 300 or 400 yards in very short steps—cannot attempt to take steps of the ordinary length, even at a very slow rate, or he would fall. He finds it more difficult to go down stairs than up them; has to look where he is going. If he turns his head round to look behind him, or even directs his eyes over his shoulder, he is in danger of falling, unless he does so very cautiously. The effect of drink is to make him worse both as regards speech and motion.

Such is an outline of his condition upon admission. It may be summarised as follows:—Evidence of paralysis, or paresis (impaired motility) of lips, tongue, and larynx, and of muscular atrophy in the muscles of the right hand with some paraplegic symptoms. When questioned, the day after admission, as to his complaint, he replied that he suffered from “weakness of the limbs, was quite nervous, and had scarcely any power of his back.” He certainly was very tottering on his limbs—so much so, that he was afraid to attempt to walk, for, as he said, half a cork would throw him down. A stick is no assistance to him, and he does not carry one, as he has not power enough in his right hand to use it, and the efforts to manage the stick only add to his difficulties. When lying on his back in bed he was asked to sit up; after many wriggling attempts to do so he was obliged to give up the task. If put sitting on the floor he cannot rise from it. The weakness of his tongue was evident from his inability to form such sounds as r, s, sch, and the words “frozen” and “school” he could not pronounce. He is badly able to draw his pipe, and in consequence smokes very little. His reflexes were tested carefully by Mr. (now Dr.) E. E. Lennon, who reported as follows:—*Superficial reflexes*—*Plantar* (both) exaggerated; muscles of feet and legs contracting. *Cremasteric* present on right side, much less marked on left. *Abdominal*, present on both sides, perhaps slightly exaggerated. *Epi-gastric*, present but not well marked. *Scapular*, absent. *Gluteal*, slight on either side, more obvious on left than right side.

*Deep reflexes*.—*Ankle clonus*, absent. *Knee jerk*, normal on right side, exaggerated slightly on left. *Pupil reflex*, normal.

He remained five weeks in hospital. As I could hold out no hopes of improvement I did not press him to remain longer. While in hospital he got two or three bad falls in walking from his ward to the W. C., owing to his slipper catching in the head of a nail which did not project more than an eighth of an inch from the floor. He said his power of swallowing was improved by galvanism, the negative pole over the laryngeal region made marked cathodic closure contraction which retracted his tongue and produced movements of deglutition. During his stay!

took liq. sod. arsen., 80 m. + syr. fer. iod., 3 viij. + inf. calumb. ad. 38, 3ss. t. d. When he left hospital, 15th April, 1883, he considered that he had gained more confidence in swallowing and in walking, but that the weakness of the right arm and leg had increased.

Nine months afterwards the doctor of the town he lived in wrote to me as follows, in answer to inquiries about him :—

“T. C. has got gradually worse and worse since the time he left the Meath. At present he is the most deplorable object that could possibly be seen. He is scarcely able to stir hand or foot, and has altogether lost the power of speech—in fact, he is not able to utter a single intelligible sound. The flesh might be said to have melted off his bones, and he is virtually a living skeleton. His lower jaw hangs open, and the saliva is continually dribbling from his mouth. I do not think from his present condition that he can live more than a few days.”

This was written on 24th Jan., 1884, and he died on the 14th of March the same year.

In this case, it appears, the bulbar symptoms were a complication or sequela of progressive muscular atrophy, the disease having extended from the anterior cornua of the cord up to the medulla. The history starts with wasting of the thumb-muscles, March, 1882, lingual and oral symptoms setting in at Christmas of the same year. In other cases the disease, initiated in the bulb, descends to the anterior cornua of the cord, and the muscular atrophy is a late event in the clinical history.

In the first of the two bulbar cases the disease had not, while under my observation, extended downwards, as there were no positive signs of muscular atrophy.

This affection, in the orthodox form of chronic progressive bulbar paralysis, is considered to be generally due to atrophic changes of the ganglion cells of the gray nuclei, on the floor of the fourth ventricle, and in most cases to be of myelitic origin.

The nucleus of the hypoglossal nerve appears to be the starting-point of the disease, the nuclei of the spinal accessory and vagus are next attacked, while the disease does not extend in all cases to the nucleus of the glosso-pharyngeal. The nuclei of the facial are attacked at a very early stage, especially those which are connected with the inferior branches of the nerve, and which have been named the accessory nuclei of the facial. It is certainly remarkable what an amount of paralysis the orbicularis oris suffers from in contrast with the orbicularis oculi.

There is a form of this affection in which the symptoms come

on suddenly, which is probably due to a slight hæmorrhage into the medulla oblongata, or to embolism, or thrombosis. The onset of this form is different from that of the typical variety in which the symptoms creep on gradually and stealthily, a slight affection of speech being usually the first symptom to attract attention. In the sudden, or, as it is called, the acute, form, the resulting paralysis is frequently unilateral, or more pronounced on one side than the other; while progressive bulbar paralysis is always gradual in its onset, and the paralysis is uniformly bilateral.

The disease is much more common in men than in women, the proportion being two of the former to one of the latter. Of five cases, of which I have kept notes, only one was a female.

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**ART. XXI.—*On Varicose Veins and their Treatment by Operation.*<sup>a</sup>**

By KENDAL FRANKS, M.D. Univ. Dubl.; F.R.C.S.I.; Fellow, and Member of Council, of the Surgical Section of the Academy of Medicine in Ireland; Fellow of the Royal Medical and Chirurgical Society of London; Vice-President of the University Biological Association; Surgeon to the Adelaide Hospital and to the Dublin Throat and Ear Hospital, &c.

ALTHOUGH the subject of varicose veins may perhaps be considered somewhat threadbare, the condition itself being of common occurrence, I consider that I am justified in bringing it before you this evening, because the result of their radical treatment hitherto has been so unsatisfactory that any procedure which promises to give more certain and lasting results, without, at the same time, increasing the risks of dealing with them, must be of interest and claim special attention. The urgency with which the authors of works on general surgery deprecate any operative procedure, except in most severe cases of the disease, sufficiently proves that hitherto no method of treatment has been so far fruitful of good as to warrant its general adoption. In discussing this subject, then, I shall deal chiefly with operative measures, and shall only allude in a passing manner to palliative treatment. And let me here, once for all, state that the majority of cases coming under notice are not cases for operation, but can be effectively dealt with by such measures as rest, bandaging, the elastic bandage, or stocking, and so forth. I shall presently state the cases which, in my opinion,

<sup>a</sup> Read before the University Biological Association at the meeting, March 25, 1886.

are best treated by operation; but before doing so it will be necessary to consider what varicosity of the veins consists in, and to what the condition is due.

Varicose veins, then, may result, when the veins are no longer equal to the pressure of the blood within them. This may follow from two causes—one *extrinsic*, when from some remote cause an unusual amount of pressure is thrown upon the veins, such as pressure upon the iliac veins, due to overloading of the intestines or to some abdominal tumour, or, as in some forms of heart disease, cirrhosis of the liver, and such other obstructions to the free course of the blood in the veins. Or the cause may be *intrinsic*—that is, for some reason or other, the veins have lost their tone and their elasticity, and are no longer capable of resisting the normal pressure from within. It is not easy to account for this loss of tone and elasticity in the veins, which has been attributed by some to an hereditary predisposition, and by Sir J. Paget is stated to co-exist with an indolent temperament or with a debilitated condition of the general health (*St. Bartholomew's Hosp. Reports*, Vol. II.). Others look upon the condition of the veins as the result of a chronic phlebitis, analogous to chronic endarteritis, causing such an alteration in the walls of the vessels themselves that they lose their elasticity and become prone to dilatation, as we are accustomed to see it in aneurysm. This chronic phlebitis is further supposed to be due to some nervous condition which results in paralysis of the vaso-motor nerves supplying the small vessels which nourish the veins. From this results hyperæmia of the coats of the veins, ending in chronic phlebitis.

Whatever the agency at work may be, the result is the same—namely, that the balance between the elasticity of the walls of the vein and the intravenous pressure is lost, and gradual dilatation and distension of the veins ensue. This loss of balance will be felt, of course, wherever the pressure is greatest; and these situations are those, in the dependent parts of the body, where the column of blood is the longest. If we take a long U-shaped tube and almost fill it with water, the fluid in one limb will rise to the same height as in the other, but the pressure of the fluid on the sides of the tube will be greater the nearer we approach the base of the U—that is, the longer the column of fluid is. So it is in the veins. The column of blood in the veins is supported by the column of blood in the arteries, but the pressure in the veins will depend on the length of the column of blood it has to support.

No doubt nature 'provides a means of taking off this excessive pressure by supplying the veins with valves, so that under normal circumstances the vein has only to support the column of blood that lies between two pairs of valves. But nature has also endowed the veins with the power of distending, so as to be able to accommodate an increased quantity of blood, should there arise any temporary obstruction to its onward flow through the heart. Now, when the veins so dilate, the valves within them are drawn apart and so allow of regurgitation. This temporary distension of the veins and insufficiency of the valves is quite a normal process. But suppose that the obstruction to the onward flow of the blood, due to one of the extrinsic causes, is permanent, or that the condition of the vein walls is such that after distension their elasticity has become so impaired as to prevent them returning to their usual size, then the valves remain permanently apart, and are no longer capable of supporting the column of the blood. This throws an extra weight on the valves below, and these again yielding, the functions of the veins become more and more impaired. Now, in obedience to the general law in the body, that when a part loses its function it gradually wastes, so in the veins we find that the valves, being unable to accomplish their purposes, gradually atrophy, and may ultimately either disappear altogether, or their former existence be only recognised by thin fibrous bands on the inside of the vein. Hence it is, as Gay says, that "as a rule, veins that become varicose are destitute of valves."

Thus it happens that the veins below the original site of lesion have a permanently increased pressure of blood thrown upon them, and this alone will eventually cause them to become varicose, even though they were themselves originally healthy, and although the original cause of the obstruction to the circulation may have disappeared. Let me here give an illustration of what I mean. Suppose that the original cause of increased pressure in the veins of a limb has been due to constipation and the pressure of intestinal accumulation upon the iliac veins—suppose that this has continued long enough to cause varicosity in some of the veins of the leg, say below the knee; after a time the valves in these veins have become atrophied and the veins permanently dilated—now, suppose that under proper treatment the constipation has been cured, will this allow the veins to resume their normal condition? No; on the contrary, this very destruction of the valves has thrown a permanently increased pressure on the veins below them, and this



alone will cause them to undergo the same process, unless means be adopted to relieve them of the unusual pressure.

Some doubt has been expressed as to whether the deep veins in the leg—that is, the veins running between the muscles and those which emerge from the muscles themselves—are liable to varix like the superficial veins. I think Verneuil has satisfactorily proved that they are, and that in the majority of cases the subcutaneous and the inter-muscular veins suffer equally. It is, however, doubtful that the intra-muscular veins join in the lesion. It has been pointed out by Briquet that the place where varix first appears is at those points where large veins, emerging from the muscles, empty themselves into either the superficial or the deep veins, and that in advanced varicosity of the veins these are the places in which the disease is most aggravated. Now, the fact that for many years it was doubtful whether the deep veins ever became varicose, shows that, even when they are, the condition gives rise to comparatively little inconvenience. This, I believe, is due to the fact that, lying between powerful muscles which are constantly contracting, they are so supported and protected that, even in a varicose state, their function is preserved. And this will also explain why they do not suffer, as a rule, as often as the superficial ones, nor to the same degree; and will also explain why the intra-muscular branches themselves scarcely ever, or but rarely, are found to be in a varicose state. But it is quite otherwise with the subcutaneous veins. The skin alone protecting them on one side, can afford them but poor support, and therefore all the evil effects that can follow from varicose veins find their fullest development in connection with these superficial varices. As a result of the obstructed circulation in the veins of the leg, we find the circulation in those parts of the skin and subcutaneous tissue, from which the venous radicals arise, is impeded. The result is, the skin no longer obtains the quantity or the quality of nourishment that it should, and we then see it become discoloured. From exudation from the obstructed veins the tissues around them become infiltrated, and we find the skin of the part becoming brawny. It is a process of slow starvation. Keep it unrelieved a little longer, and the tissues, no longer receiving enough nourishment to maintain vitality, die, or, in other words, the skin sloughs, and an ulcer is left—that well-marked sore, with its unhealthy or sloughing surface, and hard, elevated, brawny edges, which is commonly, and, I believe, very properly, called a varicose ulcer.

Note, then, the sequence of events. A vein is no longer able to support the pressure of blood within it, either because it has suffered from some *intrinsic* cause which has destroyed its elasticity, or because the pressure from above has from some *extrinsic* cause become too great. It becomes varicose. Let me digress here for a moment. Some of you have seen varicose veins excised. One thing must have struck you, if you have seen many such cases, and that is that in some cases the veins are excessively thin-walled—so thin, indeed, that it is a wonder they have not burst long ago; and under these circumstances it is often a matter of considerable difficulty to dissect them out or to apply a ligature. Again, in other cases you will have noticed that the veins are extremely thick; their walls are hypertrophied. In one case, I remember excising a portion of the saphena vein just above the bend of the knee. It was as thick and as large as the iliac artery, and admitted the tip of my little finger. I found it very difficult to realise that it was a vein I was dealing with. Now, I believe we shall find that in these varying conditions we have a clue as to the cause of the varicosity of the veins. These tough, thick-walled veins, I believe, are veins which have become varicose owing to *intrinsic* causes. They have, from some cause or other, been the seat of a chronic phlebitis, and that phlebitis has given rise to hypertrophy of the coats and loss of elasticity. On the other hand, the thin-walled veins have yielded to *extrinsic* causes. They have simply had a burden thrown upon them they were unable to bear, and they have passively become dilated, and consequently thinned.

Now, to return to the sequel of events following on a vein in the leg becoming varicose. The circulation in the part is checked, the nourishment of the part is therefore seriously interfered with. The skin first becomes discoloured and suffers from a form of eczema. Finally, the skin supply is so deficient that the part sloughs, and we have, as a consequence, an ulcer. Added to this, that the vein wall may suffer, and terrible hæmorrhage may ensue not only from the lower radicals but from the trunk, in which there are no valves to check the backward flow. Now, under these varying circumstances, what treatment should we adopt? In the early stage, when as yet the vein is to a limited extent involved, but before the skin has suffered in any way, there can be no question that palliative measures should be adopted, and of these, in my opinion, the best is the elastic bandage. Its object is

to yield that support to the veins which their walls are unable to afford; and by such means, provided the offending cause be removed, and provided that the valves have not been destroyed, we may even hope for a cure ultimately. I will go further and say that in old and very debilitated subjects such palliative measures are preferable to operation, under all circumstances. Again, if the varicosity of the veins in the legs be due to an irremediable extrinsic cause, operation is obviously excluded; as, for instance, if the condition be due to pregnancy, to pressure of an abdominal tumour on the iliac veins, to disease of the heart, to cirrhosis of the liver, and so forth. Therefore, in all cases before operative measures be adopted, it will be necessary to satisfy ourselves first that such causes do not exist. From these exceptions we may deduce the cases in which I believe operation is advisable. Firstly, the varices must be due to intrinsic causes, or to remediable extrinsic causes, such as constipation, the pressure of an ill-fitting truss, tight garters, or too long standing. In these latter cases the cause must be removed. Then, again, the patients should be young, or healthy adults; and let me here say that, in my opinion, comparatively slight varicosity may induce us to operate in a young subject, which in an advanced adult would not justify us.

Subject to the conditions already laid down, if, in a healthy adult, we find a varicose condition of the veins accompanied by an ulcer, or with a brawny condition of the skin, or with eczema, if it be sufficiently extensive to give rise to pain or discomfort, I think operation is the best treatment.

Now, what do we hope to attain by operation? Complete obliteration of the diseased vein. This has been sought after in all the many and diverse methods which have from time to time been recommended. By occluding the vessel we at once remove the pressure on the parts below caused by the long column of blood in the patulous vein. The effects of this on the parts below are obvious. It relieves at once the cutaneous and subcutaneous circulation, and will therefore not only help to cure, in the most efficient manner, brawiness and eczema of the skin, and ulcers, but it removes the chief factor in causing the more distal veins to join in the varicose condition. Nor need we fear that by closing these blood-channels we shall increase the blood stasis. The venous anastomoses are so free, not only between the superficial vessels themselves, but between the superficial and deep veins, that the utmost we do is to divert the blood from the subcutaneous courses

into the inter-muscular and deep veins of the limb, where, as we have seen, they have more external means of support than those which run beneath the skin.

I have stated that all the various operations recommended for the cure of varices aim at obliteration of the vein. One exception deserves passing notice—that of Mr. Herapath, who proposed division of the falciform process of the saphenic opening in order to remove a supposed pressure on the femoral vein. After some experience of its effects, he subsequently declared that he had lost confidence in it, as the benefit he supposed he had attained was but transient. The various methods employed for procuring occlusion of the veins may be divided into—1. The production of coagulation by the external application of caustics. 2. Subcutaneous constriction of the veins, alone or accompanied by injection into the veins, or by subcutaneous division. 3. Excision of the veins. The first of these methods was, in these countries, first tried by Sir B. Brodie; he employed *potassa fusa* along the track of the veins, but his experience of it was not encouraging. It was subsequently re-introduced by Mayo, whose name is commonly associated with it. He used nitrate of silver, but shortly afterwards, in France, Vienna paste was substituted for this. The late Dr. Samuel D. Gross, of Philadelphia, speaks of this method as the safest, as well as the most effectual. This is the description he gives of it, and I think we can hardly say that the proceeding is not a severe one. The method “consists in making a number of eschars with equal parts of caustic potassa and quicklime, converted into a consistent paste with alcohol. Of this, a portion of the size and shape of a three-cent piece, only much thicker, is placed directly upon the enlarged and tortuous vessel, at intervals of three, four, or five inches, and allowed to remain for fifteen minutes, at the end of which the skin and connective tissue will be found to be thoroughly destroyed. The paste is now removed, and the parts, carefully washed with vinegar to neutralise any of the alkali that may still adhere to the surface, are covered with an emollient poultice, for the purpose of promoting, first, the separation of the eschars, and, secondly, the development of granulations. The cure is usually somewhat tedious on account of the length of time required to heal the issues, but it possesses the great advantage of being entirely free from danger, and always perfectly successful.” I think I shall presently be able to show that we can to-day do much better for our patients in the way of curing them expeditiously and without pain. The

second method is the subcutaneous method; and I shall very briefly enumerate the various modifications. Sir Everard Home tried ligature of the trunk of the saphena vein on the inside of the knee, and it was adopted by Sir B. Brodie, Sir A. Cooper, and others; but as fatal results followed in many cases, the method speedily fell into disrepute. More lately, subcutaneous ligature with a metallic wire has been practised by Dr. Levis in America, and is said to have proved a safe expedient. The wires separate spontaneously in from two to three weeks, especially if they are occasionally tightened.

In 1817 Sir B. Brodie recommended subcutaneous division of the diseased vein. His comment upon it later was that it appeared as "though the veins generally healed without becoming obliterated; and in case the cluster became obliterated, others took its place, and no benefit ensued." Add to this, that suppuration followed in several instances, and even death. Constriction of the vein by means of a needle passed beneath it, crossed by a figure-of-8 ligature in front of it, was first suggested by Davat, of Aix, and was subsequently advocated by Sir W. Fergusson, Velpeau, Gay, and others. Mr. Lee modified this by passing two needles at the distance of three-fourths of an inch from each other, the vein being pressed against the pin by indiarubber ligatures and divided between them subcutaneously. Previous to adopting the method of antiseptic excision of varices, I several times practised the needle operation, and I was anything but satisfied with the results. I found it caused a considerable amount of irritation, often inflammation and ulceration, and frequently without any beneficial results. On two occasions I had to have recourse subsequently to excision, to cure the veins on which I had twice in each case tried subcutaneous constriction with needles. Moreover, this method is not without its risks, for on several occasions—one of them reported by Mr. Davies-Colley (*Guy's Hosp. Reports*, 1875, p. 431)—the needle, while passing beneath the offending vein, transfixed a deeper but unnoticed vein which ran parallel with it. Mr. Howse, who is a strong advocate for the antiseptic excision method, thus speaks of the objections to the subcutaneous needle:—"Any operation which depends for its success on continuous pressure on such a sensitive structure as the skin, involves much more pain and constitutional disturbance than a clean cut wound. Indeed, the late Sir William Fergusson recommended that the pins should be allowed to remain, with the silk on them, until they had 'excited considerable swelling

and slight ulceration;’ stating, further, that ‘in some instances they might be left until they had separated by ulceration *through both vein and skin.*’ And he justifies this recommendation by recording that in one case he compressed an inch or more of the enlarged internal saphena vein, and on withdrawing the pins, before much inflammation was excited, found that the blood again circulated through the vein in a scarcely diminished volume” (*Guy’s Hospital Reports*, 1877, p. 462).

The cure of varicose veins by injecting into them perchloride of iron was originated by Dr. Pravaz, of Prague, and has been tried extensively in Germany, France, and England. Mr. Erichsen describes it as a dangerous practice, and in Germany it was “excluded by the phantom of embolism,” as Dr. Ellinger said, when he endeavoured to resuscitate it. He could not, however, prevent some disastrous results, “but,” writes Mr. Gay, in his work on “Varicose Disease,” “Herr Ellinger is nothing daunted, upon the common principle that ‘accidents will happen,’ or, as he puts it, ‘death will follow the treatment of other trivial cases’” (p. 146).

Lastly, I come to the treatment by excision. This is no new method; the only novelty in it consists in providing for and anticipating the dangers to which the method was liable from septic contamination of the wound. Excision was practised as long ago as the days of Celsus, who “drew a distinction between straight and convoluted varices; for the first he recommends that they should be exposed by cutting through the skin, and then destroyed by the actual cautery; whilst the latter he at once cut out with the knife” (“Holmes’ System,” 1870, Vol. III., p. 387).

Severinus and Fallopius tied the vein in two places, and excised the intermediate portion. This method of cure, however, so frequently led to disastrous results that till within the last few years we find it universally condemned. Thus Dr. Gross writes:—“Excision and direct exposure of the diseased vessels are too dangerous to be practised, being extremely liable to be followed by phlebitis, erysipelas, and pyæmia. My conviction is that no surgeon should ever expose a patient to such risks” (Gross, “System of Surgery,” 1882, Vol. II., p. 1,047).

Under the former methods of dealing with wounds these dangers and risks were by no means imaginary, and it was not without reason that surgeons held veins in most respectful awe. But these very dangers are the dangers which the antiseptic system, when

fully carried out, abolishes, and under its protective influence veins are treated with no more respect or fear than any other tissue of the body. I have twice tied up an opening in the internal jugular vein occurring accidentally during the course of an operation, and once in the axillary vein when removing cancerous glands, without any trouble following; and in the treatment of varicose veins in the legs I excise them with the conviction that I am thereby exposing the patient to no greater risk than if they were left alone.

I will not delay you by entering into the details of the cases in which I have operated. I have full notes of eighteen cases which I have treated by antiseptic excision, besides several cases the notes of which I have not preserved. In no case have I seen "phlebitis, erysipelas, or pyæmia" follow as a result. Let me describe to you the method of operating. The patient being placed under the influence of ether, I begin by shaving the parts where I purpose to make the incisions. The skin is then carefully washed with corrosive sublimate solution; sometimes oil of eucalyptus is also used. I then usually fasten a band round the limb, immediately above the knee, so as to distend the veins sufficiently to be able to trace them accurately. I know some surgeons prefer to apply an Esmarch's bandage to empty the limb of blood, having previously marked out the veins with ink. This I do not think a good plan. In the first place, the ink is very liable to be washed out during the cleansing process; and, moreover, it is easier to secure *all* bleeding points when they can be seen. If the veins in the limb are extensively varicose, it is much better practice to excise the chief radicals at intervals, removing two or three inches at each place, than to attempt to excise a long piece of vein. Nothing is gained by the more extensive incision. Having selected the place for incision, a clean cut is made through the skin, and almost immediately the swollen vein appears. The subcutaneous tissue over it is divided on a director. Should the vein be cut, it is at once seized with Sir Spencer Wells' forceps. A strong cat-gut ligature is passed round the vein at its lower end. The vein above this is seized with forceps and the vein divided between the forceps and the ligature. It can then be easily pulled out of its bed. Any radicals going into it are ligatured and cut off; finally, the vein is tied at the upper angle of the wound and the piece excised. When the veins are tough with hypertrophied coats the proceeding is very simple; but when the veins are thin, especially if adhering to the skin, a good deal of care and patience are

required. The wound is irrigated with corrosive sublimate solution, 1 in 2,000, a little iodoform dusted over it and the edges brought together. In my earlier cases I used to insert a drainage tube, but I now think it is quite unnecessary. The wound is then enveloped in some of the antiseptic dressing, whilst a second and a third piece of the vein is treated in the same way, if necessary. If both legs are involved, the second leg is treated in the same way at the same time. Both legs are then bandaged from the toes to above the knee. As a rule, the dressings are left undisturbed for eight or ten days, and when removed we generally find the wounds healed by first intention. In a few cases some supuration occurred, but this was generally traced to some deficiency in the dressings, at a time when the dressings were improperly prepared. This only delayed the process of healing, but in every case the asepsis of the wound had been sufficient to protect the veins from contamination.

Now, let me say a few words as to the general effect of the operation. The beneficial effects have been in many cases so marked as to dispel all doubts as to the efficiency of the cure. In one case the patient was a man, aged thirty-six, who became perennially an inmate of the Adelaide Hospital to be treated for varicose ulcers. He refused several times to have any operation performed. At last I told him I would not take him in again for varicose ulcers, unless he would consent to have an operation on the veins tried. Two years ago he presented himself again, with the ulcer as bad as ever, the skin brawny and discoloured, the edges of the ulcer hard, elevated, and inflamed. He consented to an operation for the cure of the veins. I kept him in bed for several weeks, and treated the ulcer until it was about the size of a florin, and was quite healthy. I then had him placed under ether, and I excised portions of those veins which seemed to be chiefly connected with the ulcer. I may mention that all the veins operated on were above the ulcer—that is, on the side nearest the heart. The operation was performed as usual, and the dressings applied were not disturbed for a fortnight. They included the ulcer. When they were removed, the ulcer was found to be perfectly healed beneath them, and all the incisions—three in number—had healed by first intention. I allude to this case because it goes to prove that these ulcers are caused by the pressure in their efferent veins.

Early in November last I operated on a young gentleman, aged



twenty-five, the subject of extensive varicose veins in the right leg. The saphena vein at the bend of the knee was very large. He had previously been operated on by a surgeon in Dublin twice by the subcutaneous needle method, for the obliteration of the saphena vein at the knee. On each occasion three needles were passed beneath it at intervals of half an inch, and yet when I saw him this vein was as patent and as varicose as if it had never been touched, though the skin over it showed marks of where it had been constricted. I excised portions of the three most aggravated varices I could find. The wounds all healed by first intention. I saw this patient to-day. The veins operated on are all obliterated. He told me he had lost the bursting feeling in his leg from which he used to suffer, and that he had discarded the elastic stocking. The veins unoperated on remain varicose, neither better nor worse than when I saw him in November, and he is so pleased with the former operation that he wishes all the veins to be treated in a similar manner. This case illustrates the superiority of excision over the constriction method.

Before bringing this paper to a close, let me say a few words as to the permanency of the cure. Most of the writers on this subject are of opinion that the disease is incurable, and that the most to be expected from it is temporary relief, and, in fact, that the game is not worth the candle. Let us observe, however, that these conclusions are drawn from those other methods of treatment, which I have shown are in many ways defective. Since antiseptic excision has begun to be adopted, within the past ten years, we find on record many cases of cure, which have lasted over one, two, or more years, and this has been my own experience. If the varicose condition of the veins is due to a cause which we cannot hope to rectify—an extrinsic cause—we cannot expect operation to be successful. But when we can remove the cause, and when, at the same time, we treat the effect, I consider that we have just grounds for assuming that the cure will be radical. Two years ago I operated on a young man, aged twenty-six, for extensive varices of one leg. A year and a half later he wrote to me to express his great delight at the permanency of the cure; that since the operation had been performed he had been able to take long walks, to stand the greater portion of the day without the slightest inconvenience, or without any sign of fresh varices appearing. I could point to many similar cases, but I feel I have already detained you so long that I will content myself with

adding that I hope I have succeeded in impressing you with my own conviction that in antiseptic excision, in suitable cases, we have a means of treating varicose veins which offers far better and more permanent results than any other method hitherto devised, and that with ordinary care it is far safer.

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ART. XXII.—*The Use of the Curette in the Diagnosis and Treatment of Diseases of the Uterine Mucous Membrane.*\* By W. J. SMYLY, M.D. Univ. Dubl.; F.K.Q.C.P.; Gynæcologist to the City of Dublin Hospital.

BEFORE entering upon the special subject of the use of the curette, in the diagnosis and treatment of diseases of the uterine mucous membrane, permit me to guard against a possible misapprehension. I do not wish it to be supposed that because I have limited my remarks, as far as possible, to the use of a single instrument, that therefore I would limit our means of diagnosis to its use; on the contrary, it is against such exclusiveness that I wish to contend. It certainly seems strange that, although the sense of sight has long since been brought to bear upon the diseased conditions of the uterine mucous membrane, yielding, as might have been expected, most important results, we should almost have ignored this method of diagnosis, and have practically limited ourselves to the sense of touch. In proof of this I would refer to our text-books or to Dr. Edis' paper, "On the Exploration of the Uterine Cavity in Cases of Metrorrhagia," read at the meeting of the British Gynæcological Society on the 9th of last December. There he advises—and all who joined in the subsequent discussion appeared to agree with him—"in all cases where hæmorrhage from the organ persists unnaturally," to dilate the cervix and explore the cavity with the finger. Now, my present object is to state my conviction, and, if possible, to prove that this routine practice of dilating the cervix for diagnostic purposes is unnecessary; and that if an intelligent use of the curette be substituted for it, dilatation will be required in exceptional cases only. I do not, however, deny that the use of the curette for diagnostic purposes is recommended in our text-books, or that it is occasionally employed for this purpose; but the teaching in these countries appears to be this—first to explore with the finger, and if this fail, then try the curette;

\* Read before the Obstetrical Section of the Academy of Medicine in Ireland, Friday, April 2, 1886.

whereas the contrary course appears to me advisable—namely, first the curette, and this failing, then the finger.

Let us now submit these two methods to the admirable tests adopted by Dr. Edis in the paper above referred to—first, efficacy; second, harmlessness; and thirdly, facility.

Let us apply them, in the first place, to the method of examination by the finger. Is it the more efficacious? No doubt, in a large uterine cavity, with soft and yielding walls, gross pathological changes, such as polypi, carcinoma, or pulpy and thickened membrane, can be easily felt. But the conditions are very different when the uterus is firm and unyielding, and when its cavity is but slightly enlarged. In such a case the tactile sense is greatly impaired by compression, and it will often be found impossible thoroughly to explore the cavity. But, even under the most favourable conditions, who would attempt to distinguish by touch alone such affections as retention of the decidua, fungous endometritis, malignant adenoma and sarcoma; or catarrhal, interstitial, and atrophic endometritis—affections our knowledge of which has been almost entirely derived from the use of the curette and the microscopic investigations of such men as Dr. C. Ruge of Berlin?

As to harmlessness, I should not wish to exaggerate the risks which attend upon the dilatation of the uterine canal; when properly carried out they are not very great, but in pre-antiseptic times this was not so; and considering the very loose ideas still prevalent as to the necessity for minuteness in antiseptic details in the minor gynæcological operations, I think that a rule necessitating the dilatation of the cervix, and the introduction of the finger in all cases of severe intra-uterine disease, has caused, and will cause, much suffering, and even loss of life. There is also—and this appears to me to be a point of much importance—a large class of cases in which the dangers attending this process are so great as almost to preclude its employment—as, for example, those in which there is inflammation in the uterus itself, or in the tissues surrounding it. In these cases the manipulations attending the dilatation and examination with the finger would probably lead to an aggravation or rekindling of the mischief. The case of a patient at present in the City of Dublin Hospital illustrates this very well. She was twice admitted into the Rotunda during the time that I was Assistant to the Master, suffering from severe menorrhagia. The uterus was dilated with sea tangle and mopped out with fuming nitric acid, and with excellent results. But upon

a subsequent occasion this was followed by a severe attack of inflammation, the results of which can still be felt in the neighbourhood of the uterus. Dilatation in this case would be hazardous; but I had no hesitation in using the curette, and with excellent effect.

That the methods of dilating the cervix are not altogether satisfactory may be gathered from the number in vogue at the present time, and the variety of instruments which have been devised for carrying them out. Some of these methods are difficult and some dangerous; some involve a bruising and lacerating of the tissues, and others intense suffering to the patient, and loss of time to the practitioner. Now, although these imperfections are not present to so marked a degree as to counterbalance the important advantages of correctness in diagnosis, were dilatation necessary for its attainment, yet they are quite sufficient to prevent our resorting to this proceeding where shown to be unnecessary for such a purpose.

If, now, the same tests be applied to the use of the curette, it will be found, in the first place, to be eminently efficient as a diagnostic agent. With this object it should at first be passed lightly over the surface of the membrane lining the interior of the uterus, whereby the condition of the latter can be readily appreciated, whether smooth and even, or rough and irregular, soft like velvet, or harsh and grating. I have never myself detected a polypus in this way; but Dr. Fritsch says "that it is impossible to discover and to remove small polypi without having felt them will only be believed by those who have no experience in this method" (*Die Lageveränderungen und Entzündungen der Gebärmutter*, p. 1,017). The cutting edge of the curette should now be used, and the entire mucous membrane removed by firm bold strokes. As the instrument is occasionally withdrawn during this process, it will be followed by long stripes of mucous membrane, either thin and translucent, as in the normal and in simple catarrhal conditions; or thick, succulent, and semi-opaque, as in hyperplasia. In malignant affections, hard, friable particles, often of a whitish colour, will probably come away. This alone is generally sufficient for the diagnosis, but occasionally recourse must be had to the microscope. It has been denied that the particles of membrane thus obtained are sufficient for the latter purpose, or that a diagnosis can be made in this way; and I have no doubt that it is often a very troublesome and tedious task, but that it can be done is

certain. In nine cases published by Düvelius,\* the diagnosis was made by this means alone; in two it was carcinoma, in two malignant adenoma, in one sarcoma, and in four simple adenomatous endometritis. In all the organ was extirpated; in the four latter, on account of uncontrollable hæmorrhage, and the diagnosis confirmed by the subsequent examination of the specimens obtained. In two cases of my own, in which I felt doubtful as to the nature of the disease, I had the scrapings examined microscopically, and this important point determined. I have, therefore, no doubt that a certain diagnosis can be made in this way, provided the disease involves the mucous membrane.

Now, as to the second test—harmlessness. Certain dangers naturally suggest themselves as likely to attend this process; and especially, that the mucous membrane having been removed, it would be replaced by cicatricial tissue, and thus sterility would result. Reasoning from analogy, we should expect this to occur, but analogy does not hold good in this instance. The uterine mucous membrane differs from every other in its rapid and complete regeneration, as is well illustrated by two cases of Dr. Martin's. The patients, who were both approaching the climacteric, had been thoroughly and repeatedly curetted, and cauterised, for metrorrhagia, but without benefit. They were at last reduced to such an anæmic condition that, as a last resource, the uterus was removed. In one of these cases the last scraping took place two, in the other, four months previously. The microscopic preparations obtained after extirpation showed no trace of the previous energetic curetting. The newly-developed membrane was related to the muscular and intermuscular connective tissue in the usual manner, nor was a cicatrix to be found anywhere in the mucous membrane, or between it and the muscle. This is what might be expected if the new formation grows from beneath and not from the sides. Schröder believes that it grows from the remains of the glands which are left by the curette. Theoretically, therefore, there is no reason why nidation should be interfered with, but, on the contrary, it should rather be favoured by the removal of a diseased membrane and its replacement by another which we have every reason to hope may prove a healthy one. Now, as a matter of experience, it has been found that this is the case. Dr. Düvelius found that out of the patients operated upon during the four years between 1879–1883 by himself and Dr. Martin, 60 were known to

\* *Zeitschrift für Geburtshülfe und Gynäkologie.* Bd. X.

have become pregnant. In 11 of these the operation was performed on account of incomplete abortion; in the remaining cases, for disease of the endometrium or for sterility. Sixteen times the diagnosis was confirmed microscopically. Out of 49 patients 32 had previously borne children; 17 had not. Out of the 60 pregnancies 6 ended in abortion. Four of these were women who were operated on in consequence of a previous miscarriage; and of these 3 conceived again after a second curetting. The exact percentage of cases in which pregnancy followed this kind of treatment could not, for obvious reasons, be ascertained. In addition to these cases, Dr. Benicke has published ten; and some of my own patients have borne children subsequent to the operation. It is, therefore, evident that, no matter how thoroughly the curetting may be carried out, the membrane will be regenerated; that this new membrane is not cicatricial in structure, and that the operation favours rather than hinders the occurrence of pregnancy.

Now, as to the third test, nothing could be much more simple than this trifling operation. The patient having been placed in the dorsal position and etherised, the thighs are flexed and fixed by Snger's apparatus, the cervix exposed by Simon's speculum and fixed with a bullet-forceps, the vagina and uterus washed out with antiseptic lotion, the sound introduced to ascertain precisely the direction of the cervical canal and uterine cavity, and then the curette is introduced and the entire mucous membrane removed; fragments and blood-clots washed out, a Braun's syringe of strong solution of iodine or perchloride of iron is injected, and the superfluous fluid is washed out again.

Before leaving this part of the subject I shall speak of a few details. Snger's apparatus is a modification of Clover's, and is better adapted to the needs of a gyncologist. I have found it of the greatest advantage, especially in private practice, where the number of assistants is a matter of consequence. With this apparatus and an irrigator upon a convenient stand, an ansthetist is the only assistant that is really necessary. For washing out the uterus I prefer the Fritsch-Bozeman catheter to any other. It is easily cleansed, insures a safe return of the fluids injected, and, owing to its conical shape, can be made to enter almost any cervix; but should difficulty be found in doing this, it is better to postpone the uterine douche until after the curetting, when, owing to a remarkable dilatation of the cervix which always accompanies this process, it will then be found to enter easily. The curette which

I prefer is either a Sims' or that employed by Dr. Martin, of Berlin, which is, I believe, very similar to that originally introduced by Recamier. It is a powerful steel instrument, with a long, oval, dull-edged spoon at both ends. The membrane is removed with the side of the spoon, which is swept around the uterine cavity; the end is only employed for scraping the fundus. This instrument I prefer when the uterine cavity is sufficiently enlarged to give it free play; but in other cases I use the sharp steel loop. A very ingenious instrument has been introduced by Freund, which combines an irrigator and curette, but I have never employed it, being perfectly satisfied with the simpler forms. The best instrument is that which removes the membrane most completely. And therefore those which, like Thomas' dull-wire curette, have been devised especially to avoid doing this, are the least efficient. A point of some practical importance is, that when the mucous membrane has been removed, and the instrument comes down upon the firm uterine wall, a harsh grating is felt, and even heard, as it is drawn over it—a sensation very different from that communicated by the soft mucous membrane, and resembles the sensation and sound produced by drawing the instrument across the extended palm of the hand.

I think, then, that we may come to this conclusion, that in the diagnosis of diseases involving the uterine mucous membrane the use of the curette will be found more efficient, safer, and easier than dilatation of the cervix and introduction of the finger.

Let us now consider this instrument as a therapeutic agent. In the foregoing remarks its action in this respect has been anticipated. It depends upon the regeneration of the mucous membrane. Why should we spend months and years in making applications to a membrane which may be got rid of in a few minutes, with a certainty of its being speedily replaced by at least as good, but probably a very much better one? That the new membrane may be diseased is true, but this objection applies to every kind of intra-uterine treatment; and speaking generally, we may say that the success which has attended any therapeutic agent in this direction has been in direct proportion to its power of destroying the diseased membrane. This explains to us the marked benefit derived from the use of powerful caustics, especially the fuming nitric acid. It would be both unpatriotic and ungrateful were I to depreciate the worth of this agent; but there are three points in which it is inferior to the curette—1st. Its use necessitates a

previous dilatation; 2nd. Its action is rendered uncertain by discharges, especially hæmorrhage; and 3rd. It destroys the tissues, upon which, in many cases, we have to depend for a diagnosis. In bringing forward some cases to illustrate the use of the curette in treatment, I at first thought of selecting some typical examples; but this method, I fear, would convey too favourable an impression of its results. I have, therefore, brought forward some cases in the order in which they were operated upon; and in order to avoid overtaxing your patience with a long list of ordinary affections, I have limited myself to twelve, which were thus treated during the summer and autumn months of 1884—a period sufficiently remote to judge of the results:—

**CASE I.**—Mrs. B., aged thirty-one, married six years. Two children and several abortions. Present illness dates from the last miscarriage, four years previously. During all this time she had been under the care of a distinguished specialist, who saw her twice weekly, and made applications to the interior of the uterus. Under this treatment she had somewhat improved.

She complained to me of profuse menstruation, lasting from eight to ten days, attended by severe pain in the back and head, and expulsion of clots. There was also a distressing forcing down feeling. Between the periods there was constant leucorrhœa. Upon examination the uterus was found enlarged three and a half inches in length, the cervix abraded, and a copious catarrhal discharge.

Believing that the ordinary methods had been fully tested during the previous four years, I determined to use the curette, and this I did on the 8th of July, 1884. After this her menstruation became regular and painless, and the leucorrhœa greatly diminished; and when I saw her a year after she looked well and strong, and told me that she never felt better in her life. She called upon me again two months ago and told me that menstruation was again becoming excessive in quantity, but never exceeded four days in duration.

**CASE II.**—C. D., aged twenty-six, a servant girl; two children; the last two years previously menstruation regular, but profuse and painful. The patient was pale and anæmic, and complained that she was unable to work in consequence of constant pain in the sacral and ovarian regions. The uterus was found retroverted and enlarged; there was copious catarrh and an abraded cervix. I had this patient under treatment for several months—sometimes in hospital and sometimes as an out-patient. The uterus was replaced and a pessary adjusted; the metritis and endometritis treated in the usual way, and the general



health attended to, but only with temporary benefit. On July 15th I curetted the uterus. Since then menstruation has been normal. She has no pain, and can do her work. I have only seen her on two occasions since to have the pessary adjusted.

**CASE III.**—Mrs. O'C., four years married; four children; last pregnancy nine weeks previously; since then continuous hæmorrhage; is weak and anæmic. Having spent a short time in trying other remedies, I came to the conclusion that the case was one of retention of the decidua, and accordingly resorted to the curette for its removal. Menstruation became normal for three periods, when she became pregnant and completed her full period. She is at present in the fourth month of another pregnancy.

**CASE IV.**—Mrs. B., aged thirty, twice married; five children by her first husband, the youngest five years of age. Her illness dated from that time. She complained of great pain at each menstrual period, of the flow being excessive and lasting from eight to ten days. On examination the cervix was found hypertrophied and fissured up to the vaginal junction on both sides, and the lips everted and eroded; the uterus soft and tender to touch, normal in position, three and a half inches in length. Many weeks were spent in this case in treating the conditions described. On Sept. 19th I scraped out the uterus, and on August 3rd performed Emmet's operation upon the lacerated cervix. I last saw her in March, 1885, and found her greatly improved; the cervix was normal in size and shape, and there was no catarrh. Menstruation, also, was less profuse.

**CASE V.**—Mrs. W., aged forty, suffering from metrorrhagia; curetted on October 4th; made a good convalescence. I have not heard of her since.

**CASE VI.**—Miss M'A., aged thirty-two. For last three years menstruation had been very irregular and painful; came on profusely one month and not at all the next. Has leucorrhœa between times, and pain in left ovarian region and back. On examination the uterus was found to be two and three-quarter inches in length, normal in shape and position. Having tried other remedies for months, I determined to resort to the curette, as much for diagnostic as therapeutic purposes. A few thin shreds of apparently normal membrane came away, and subsequently the symptoms remained unchanged.

**CASE VII.**—Mrs. C., eleven years married, no children; complained of severe pain in the sacral and left ovarian region, greatly aggravated during the menstrual flow, which was regular as to time but very small

in quantity. She had always suffered from dysmenorrhœa, which had greatly increased after marriage. She first consulted a general practitioner, who divided the cervix, but as this gave her no relief, she went to a specialist, who treated her with intra-uterine applications for nearly three years without any benefit. When I examined her the uterus was apparently normal in every way, except that the passage of the sound caused intense suffering. On October 13th I scraped out the uterus. Since then menstruation has been painless, but is still very scanty. This case puzzled me very much, and I was inclined to think that the cure of the dysmenorrhœa was due more to mental than physical causes; but on reading a most interesting paper of Prof. Schröder on dysmenorrhœal endometritis, I found there an exact description of its symptoms; and what to me was peculiarly interesting was that the use of the curette was strongly recommended as the only means of cure. The fragments removed, when examined by Dr. C. Rugé, almost always showed inter-glandular development of fibrous tissue, and the discovery of this tissue establishes the diagnosis.

**CASE VIII.**—Mrs. B.; one child in 1883; since then menorrhagia, leucorrhœa, and sterility; uterus retroverted. Having been replaced and pessary adjusted, routine treatment was adopted, with intervals, for some months. Uterus curetted Oct. 27th. Since then menstruation normal.

**CASE IX.**—Mrs. G.; has had two children, youngest six years ago. Since then menstruation had been irregular and very profuse. She was in delicate health and very anæmic; complained of pain in the back and leucorrhœa; the uterus was retroflected, and the passage of the sound caused great pain. The attempt to treat this case by local applications failed, as each attempt brought on hæmorrhage, which lasted from a week to ten days. I therefore had recourse to the curette, and with very good results. When I saw her a few days ago she told me that she was completely cured.

**CASE X.**—Mrs. S.; had three children and a series of miscarriages. She complained of menorrhagia, which was very severe, large clots being expelled with violent expulsive efforts, and a profuse leucorrhœa in the intervals. I had this patient constantly under my care since 1881, and had tried every means that promised relief, but only with temporary benefit. In 1882 I dilated the uterus, introduced my finger and explored the cavity, and then thoroughly mopped it out with fuming nitric acid; this caused a temporary improvement, but when the symptoms again became aggravated she absolutely refused to submit to the process of dilatation. On the 3rd of December I curetted the uterus, and since then she has improved much in health, and the flow has not been so excessive.

**CASE XI.**—Mrs. T., aged forty-two; hæmorrhage from imperfect abortion; uterus scraped out December 18th; menstruation became regular for awhile, but is now very irregular—sometimes three to six months interval. She is evidently at the menopause.

**CASE XII.**—M. N., aged twenty-one; one child seven months previously; since then menorrhagia; uterus curetted December 10th; menstruation became normal; have not seen her now for months.

Out of these twelve cases, seven dated from delivery and three from abortions. One was sterile and five had acquired sterility; two were not married, leaving four, one of which had reached the menopause; one has since borne a child, and two have been lost sight of; eight complained of menorrhagia or metrorrhagia—of these six were cured and two improved; three of dysmenorrhœa, all of whom were cured; all of leucorrhœa, which was less affected than the other symptoms.

In conclusion, I may state that when it is considered that most of these cases had been for months, and even years, under treatment before the use of the curette was resorted to, these results are, upon the whole, very gratifying.

**ART. XXIII.**—*Cremation : its Sanitary and Judicial Aspects.*\*

By H. C. TWEEDY, M.D., Univ. Dubl.; Diplomate in State Medicine, Trin. Coll. Dubl.; Fellow and Examiner Royal College of Surgeons; M.K.Q.C.P.; Physician to Steevens' Hospital.

THE subject of cremation has of late years been occupying an increasing share of public attention in England as well as on the Continent; and as it has now become a recognised mode of disposing of dead bodies, it may not be uninteresting to the members of this Academy to hear something of the present position of the question, the advantages claimed for this process by its advocates over other forms of burial, the arguments by which it is assailed, and the relation in which it stands to the law of England.

The subject is a large one, and might be discussed from many standpoints; but I propose to confine my observations chiefly to its sanitary and judicial aspects, as coming more especially within the domain of State Medicine.

Let us first take a glance at the rite of burial as usually

\* Read before the Sub-Section of State Medicine of the Academy of Medicine in Ireland, Wednesday, April 21, 1886.

practised in these lands, and see what influence it may have on the health of the community.

Nearly every writer and speaker on the subject of cremation makes a point of introducing the well-known words of Lucan, quoted by Sir Thomas Brown in his celebrated "*Hydriotaphia*"<sup>a</sup> (Urn-Burial):—

*"Taberne cadavera solvat  
An rogus hand refert"*

"Whether decay or the funeral pile destroys corpses it matters not." Meaning thereby, we may presume, that in either case the body must be resolved into carbonic acid, water, and ammonium, and the mineral elements of which it is composed. This, of course, must be granted; but is that all? Is it absolutely immaterial whether this inevitable change takes place by the slow and gruesome process of putrefaction, or by the safe and rapid process of cremation? That the public mind began to harbour misgivings as to the safety of the ordinary mode of burial may be gathered from the fact, that in 1849 the General Board of Health organised a Commission, consisting of Dr. Southwood Smith and others, for the purpose of instituting an examination into the state of burial-grounds in London and large provincial towns. As a result of this inquiry, such horrifying details were brought to light that Acts of Parliament were passed prohibiting intramural interment. It was then proved<sup>b</sup> "that the placing of a dead body in a grave, and covering it with a few feet of earth, does not prevent the gases generated by decomposition, together with putrescent matters which they hold in suspension, from permeating the surrounding soil and escaping into the air above and the water beneath;" and the Report further stated "that there is abundant evidence that cholera was usually prevalent in the immediate neighbourhood of London graveyards." We now know more than this—not merely that the general health of those living in the vicinity of graveyards almost inevitably deteriorates, but that distinct infective organisms when buried in the earth not only retain their vitality for years, but even multiply under favourable conditions of moisture and temperature, and when brought to the surface, either by means of earth-worms<sup>c</sup> or during the process of opening a grave, can

<sup>a</sup> Published in 1658.

<sup>b</sup> Report on a General Scheme for Extramural Sepulture. (Clowes and Sons.) (Signed)—Carlisle, Ashley, Edwin Chadwick, T. Southwood Smith.

<sup>c</sup> See Darwin's paper, read at the Geological Society of London in 1837, and quoted by Sir Spencer Wells in a paper entitled "*Cremation or Burial?*" read at the meeting of the British Medical Association at Cambridge, August, 1880.

minge with the air we breathe or the water we drink, thus conveying the specific disease of which it is the germ.

Pasteur has now demonstrated how districts in France have been poisoned by the interment in them of animals that have died of "charbon" or splenic fever, and he has shown that the one way of guarding against the spread of that disease is by burning or otherwise destroying the animals that die of it. And again, Dr. Friere, of Rio Janeiro, has shown that the same thing is true of yellow fever, the germs of which multiply to a prodigious extent in the soil of burial-grounds, and that the disease can be produced by water in which the earth of such places has been washed; and, once more, Dr. Koch has proved that cholera itself is due to a similar organism, and that this fell disease can spread under like conditions. But even supposing the absence of infection, what a slow and tedious process is the natural dissolution of the body under the most favourable circumstances. In the "earth to earth" system advocated by Mr. Seymour Haden, he states that a body buried in a porous wicker basket decays away in about six years. This process is much lengthened when the dead are buried in impermeable coffins, under circumstances in which the air cannot act upon the body to complete its combustion and allow it to pass freely into its gaseous constituents. This is a plain unexaggerated statement of facts regarding ordinary burial and its results.

The body buried is reduced to its constituent elements, no doubt, but the process is a lengthened one, extending over several years, and attended with all the loathsome accessories of putrefactive changes, as well as fraught with dangers to the living, not only by contaminating air and water, but in many cases by storing up and fostering those subtle germs of infection which, when set free, go forth each to spread the disease of which it is the peculiar ferment.

How favourably does the process of cremation contrast with this picture. Instead of dragging over a period of years, the resolution of the body is completed in a couple of hours, the gases are driven off without any offensive odour, all infective organisms are completely destroyed by the excessive heat employed (about 2000° Fahr.), and nought remains but a purified sublimate, free from every trace of organic matter and less than one-twentieth part of the weight of the original body.

Through the courtesy of Mr. Eassie, C.E., Secretary to the Cremation Society of England, I am enabled to describe the

Gorini Furnace, which is the pattern crematory chosen by the Association, and upon which the Crematorium now in use at Woking has been modelled.\* This crematory consists of a receiver, a furnace, and a chimney. The body is introduced into the crematory chamber by a door at one end. This door is then closed, and when the door at the opposite end, which separates the fuel-combustion chamber from the crematory chamber, has been drawn back, the flames from the furnace play upon the body, which is placed with the head to the flames, and these and the products of both combustions descend by a flue to a chamber underneath, from whence they ascend by side flues to the chimney, and, after the resulting gases have been purified by passing through a coke fire near the base of the chimney, they pass innocuously into the open air unaccompanied by smoke.

In Italy cremations in the public crematories built on this system occupy from one-and-a-half to two hours, in which is included the time allowed for the cooling down. The Gorini apparatus is that mainly used in Italy, and up to December 31st, 1884, 356 cremations had been effected by it in Milan, Lodi, Cremona, Varese, and Rome.

Another system, invented by the engineer, Joseph Venini, is also much used in Italy, and is highly spoken of. In Germany there are crematoria in Dresden and Gotha constructed on the Siemens principle. Various other forms of apparatus have been used in America and elsewhere; but those mentioned are at present the most highly approved.

In the presence of facts such as these I think we may fairly concede that all sanitary objections to cremation, when properly conducted, are now effectually disposed of. There are other objections, however, of a judicial or *quasi* judicial nature, of so serious a character that they demand more than a passing word.

The first of these is the possibility of premature cremation during a trance. The second, the danger that cremation may destroy traces of violence or poisoning, and thus defeat the ends of justice.

The first objection is easily disposed of. Let us hear Sir Henry Thompson, President of the Cremation Society, speak on the subject. He says:<sup>b</sup>—"There is a source of very painful dread, as I

\* Taken from a paper entitled "Cremation," read at the International Health Exhibition Conference, by W. Eassie, C.E.

<sup>b</sup> Contemporary Review. January, 1873. "Cremation: the Treatment of the Body after Death." Sir Henry Thompson.

have reason to know, little talked of, it is true, but keenly felt by many persons at one time or another, the horror of which to some is inexpressible. It is the dread of premature burial; the fear lest some deep trance should be mistaken for death, and that the awakening should take place too late. Happily such occurrences must be exceedingly rare, especially in this country, where the interval between death and burial is considerable; so the fear is almost a groundless one. Still, the conviction that such a fate is possible, which cannot be altogether denied, will always be a source of severe trial to some. With cremation no such catastrophe could ever occur, and the completeness of a properly conducted process would render death instantaneous and painless if by any unhappy chance an individual so circumstanced were submitted to it. But the guarantee against this danger would be doubled, since inspection of the entire body must precede the act of cremation, no such inspection being possible under the present system."

Several answers may be given to the second objection.

1st. A medical examination of the body previous to cremation would in the case of a large number of poisons—the minerals and mineral acids, at least—raise a strong suspicion that death had been compassed by foul means. Death from injury or from wounds would in like manner be discovered.

2nd. As far as mineral poisons are concerned, "direct experiments, instituted by M. Cadet, and repeated by MM. Dourvault and Wurst, have proved that the salts of arsenic and all other metallic poisons except mercury, which is completely volatilised, can be detected in the ashes after cremation."\*

3rd. In cases in which there was the least doubt as to the cause of death it would be possible to remove the stomach and a portion of the viscera, and to preserve them for future examination should the necessity for such examination arise.

But, independently of this, it seems scarcely fair to raise objections to cremation which may also with justice be brought against ordinary burial. For instance, most, if not all, of the organic poisons are destroyed by putrefaction, though, of course, more slowly than by fire; and again, as Dr. Cameron very justly remarked when moving the "Disposal of the Dead (Regulation) Bill, 1884"—"It is childish to think of baffling a rich and skilful criminal by prohibiting cremation, so long as you permit him to remove the viscera of his victim and preserve him by the aid of

\* *Vide* Dr. Cameron's speech. Transactions of the Cremation Society. 1885. Pp. 29.

arsenic and corrosive sublimate under pretence of embalming his remains. It is childish to think that any law can put an effective obstacle in his path as long as you permit him to transport the body of his victim to foreign countries in order that he may there bury him or burn him altogether uncontrolled by your laws." But, it may be urged, in cremating a body you destroy all possibility of such re-examination as could be made were the body buried and subsequently exhumed. Granted. But what is the real value of such an examination? In the first place, decomposition would speedily obliterate all traces of injury to the soft parts of the body. Furthermore, in many cases the difficulty of identifying a particular corpse would be extreme, more especially if it were necessary to search for it in a crowded graveyard, where scores and hundreds of bodies were buried, huddled together promiscuously; and, lastly, recent researches have shown that in the process of putrefaction cadaveric alkaloids—termed, for want of a better name, ptomains—have been found. Little is known of these, to be sure, but the knowledge that such alkaloids exist would naturally make us chary of forming conclusions based upon experiments made on animals with matters taken from the viscera of bodies in an advanced state of decomposition.

Let us now see what is the state of the law as regards cremation. It is simply permissive. A test case occurred two years ago in Wales. A man named William Price was charged with a misdemeanour under the following circumstances:—A child five months old, said to be his own, died in his house. As he did not register its death, the coroner gave him notice on a Saturday of his intention to hold an inquest on the body on the following Monday. Mr. Price, on the Sunday afternoon, took the body of the child to an open space, put it into a cask of petroleum, and set the petroleum on fire. A crowd collected; the body of the child, which was burning, was covered with earth, and the flames extinguished, and Mr. Price was brought before the magistrates, and committed for trial. The trial came on in February, 1884, when, after a very able and exhaustive charge to the Grand Jury, Mr. Justice Stephen\* summed up his remarks by saying:—"After full consideration I am of opinion that a person who burns instead of burying a dead body does not commit a criminal act, unless he does it in such a manner as to amount to a public nuisance at common law. The reason for this opinion is, that upon the fullest

\* Mr. Justice Stephen "On the Law of Cremation." Smith, Elder, and Co.



examination of the authorities, I have been unable to discover any authority for the proposition that it is a misdemeanour to burn a dead body, and in the absence of such authority I feel I have no right to declare it to be one."

This decision led to the issue of an important circular by the Cremation Society of England, from which circular the following is an extract:—

"A recent decision of Mr. Justice Stephen declares that the cremation of a dead body, if effected without nuisance to others, is a legal proceeding. Under these circumstances the Cremation Society feel it a duty to indicate, without delay, those safeguards which they deem it essential to associate with the proceeding in order to prevent the destruction of a body which may have met death by unfair means. They are aware that the chief practical objection which can be urged against the employment of cremation consists in the opportunity which it offers, apart from such precautions, for removing the traces of poison or other injury which are retained by an undestroyed body.

"The following, therefore, are the conditions on which the employment of the crematorium will alone be permitted by the Council:—1. An application in writing must be made by the friends or executors of the deceased.<sup>a</sup> 2. An ordinary medical certificate of death must be sent in by one qualified medical man at least, who attended the deceased until the time of death, and one also by a second medical practitioner,<sup>b</sup> both unhesitatingly stating that the cause of death was natural, and what that cause was. When no medical certificate is enclosed an autopsy must be made and certified by a medical officer appointed by the Society, and at the expense of the applicant or of the estate of the deceased. These conditions being complied with, the Council of the Society reserve the right in all cases of refusing permission for the performance of the crema-

<sup>a</sup> *Application from relative, executor, or friend of deceased.*—I, (Name)  
(Address) (Occupation) hereby request the Cremation Society of England to undertake the cremation of the body of \_\_\_\_\_ and I certify that the deceased expressed no objection (orally or in writing) to being cremated after death. A Medical Certificate of the cause of death is enclosed. (Signature.)

<sup>b</sup> *Certificate No. II.*—I certify that I have, in relation to the expressed desire that the deceased should be cremated, carefully and separately investigated the circumstances connected with the death. I declare that there are no circumstances connected with the death which could in my opinion make exhumation of the body hereafter necessary. Signed (the second general practitioner will sign here). Professional Title . Address . Date . This Certificate must be signed by another registered Medical Practitioner.

tion, and, in the event of permitting it, will offer every facility for its accomplishment in the best manner."

These rules are excellent and comprehensive, but at present the law has no power to enforce them.

Dr. Cameron's Bill, before alluded to, included the following important provisions:—

(a). "That places used as crematoria should be licensed, and that it should be a crime to dispose of a body by burning in any place not so licensed.

(β). "That it should be unlawful to cremate any body without a special official permit, to be issued only on receipt of a medical certificate (founded either on personal attendance during life or on a *post mortem* examination) that death undoubtedly resulted from natural causes, that the cause was so and so, and that there was no reason whatever to believe that death was caused or accelerated by foul play."

This excellent and comprehensive Bill was seconded by Dr. Farquharson, and supported by Sir L. Playfair, Sir G. Campbell, Mr. Findlater, and others, but it was unfortunately thrown out on the second reading. Notwithstanding its fate, however, one cannot but feel that were it or some enactment constructed on similar lines to become law, with the addition, perhaps, of a clause recommending (as suggested by Sir Henry Thompson in 1873) the appointment in every district of a properly qualified medical inspector, with duties corresponding to the French "*Médecin-Vérificateur des Décès*,"\* an official without whose written permission no burial can take place in Paris—one cannot but feel, I say, that we might then honestly, and without further hesitation, cast in our lot with the advocates of cremation, certain that in doing so we were outraging no law human or divine, animated with the confidence that we were benefiting our fellow-man by removing one most prolific source of disease and death, and at the same time fully satisfied that we were not opposing the course of nature, but merely anticipating by a little, and for the good of our kind, the time she herself takes in carrying out her own eternal and immutable law of "Dust to dust, and ashes to ashes."

\* The following is the text of the French law :—Code Napoléon, Article 77.—  
"Aucune inhumation ne sera faite sans une autorisation, sur papier libre et sans frais, de l'officier de l'état civil, qui ne pourra la délivrer qu'après s'être transporté auprès de la personne décédée pour s'assurer du décès, et que 24 heures après le décès, hors les cas prévus par les règlements de police."

## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

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*A Practical Treatise on the Sputum, with Special Reference to the Diagnosis, Prognosis, and Therapeutics of Diseases of the Throat and Lungs.* By G. HUNTER MACKENZIE, M.D. Edinburgh and London: W. & A. K. Johnston. 1886. 8vo, pp. 104.

THIS is a work which will, we believe, be found very useful. After a short introduction, a description is given of the sputum in those diseases in which it does not contain any specific organisms. These are—catarrh, bronchitis, bronchiectasis, putrid bronchitis, croupous bronchitis, asthma, gangrene of the lung, abscess of the lung, catarrhal pneumonia, pulmonary hyperæmia, empyema.

In several places throughout the book it is stated that the presence of elastic tissue in the sputum is no evidence of the formation of a cavity, but that it may be found in chronic bronchitis, simple catarrh, chronic laryngitis, syphilitic disease of the lungs, and other conditions.

Charcot's crystals and Curschmann's spirals are looked on as of slight diagnostic importance. All that can be said about them is that they are usually present in the inflammatory affections of the smaller bronchioles, with or without the coexistence of asthma. In describing the sputum in gangrene and putrid bronchitis no notice is taken of the very characteristic separation of the expectoration into three layers, and the expectoration in abscess of the lung is said to be characterised by having a grass-green colour. In certain cases of pleurisy and pulmonary inflammation which bear some resemblance to phthisis, the author found sputum crowded with micro-organisms. A drawing of these is given, from which no very definite conclusion as to their nature can be drawn. No description is given of the mode in which they grow in pure culture, and injection experiments have not been tried. Their presence, together with the absence of tubercular bacilli, is looked on as of favourable augury.

In the case of croupous pneumonia the naked-eye characters of

the sputum are considered to be of greater value than those derived from microscopic examination. In this we fully agree; but we think that the evidence adduced of the presence of the pneumococcus in diseases other than pneumonia requires confirmation, as the organism is not always easily recognised merely by the microscope without cultivation and injection in susceptible animals.

In phthisis the chief interest turns on the presence or absence of the bacillus. Dr. Mackenzie has never found this easily-recognised organism except in tubercular cases. Its presence is therefore considered, and we believe rightly, as positive proof of tubercular disease. He has been unable to confirm Ransome's observations of the presence of the bacillus in the breath of phthisical subjects. The number of bacilli in the sputum does not give trustworthy data for prognosis. "The most which can be said with any degree of accuracy is, that when present in excessive numbers they portend a speedy fatal result. On the other hand, when scantily present it does not follow that a given case of advanced disease may not terminate with rapidity."

"With the results now described—the presence of tubercle bacilli in the expectoration in 92 per cent. of all cases of phthisis, and their absence in every case of non-phthisical disease examined—it is obvious that their diagnostic value is of a very high standard. It may, in fact, be questioned whether any other disease possesses a single element or feature of equivalent diagnostic value. Certain it is that the bacillary test in phthisis far outweighs that dependent upon a combination of general symptoms and physical signs."

In tubercular disease of the larynx the bacilli are often present in vast numbers in the sputum.

In malignant disease of the larynx fragments of the new growth may be found in the sputum of sufficient size to be examined by sections. Good drawings are given of cancerous masses examined in this way.

The author thinks that vicarious hæmoptysis in women who suffer from amenorrhœa is very doubtful, and that in cases where this is supposed to occur the lungs will generally be found in a diseased condition.

In the chapter on the Therapeutic Indications of the Sputum, it is said that the most effectual hæmostatic in pulmonary hæmorrhage is a saturated solution of iron and alum (*sic*) in glycerine, used as a spray for inhalation. While the inhalation of antiseptic substances sometimes relieves troublesome symptoms in phthisis, it is ineffective

in destroying the tubercle bacillus, or in materially modifying the course of the disease.

Chapters on the Disinfection of the Sputum in Phthisis and on the Methods to be Employed in its Microscopic Examination conclude the work. The text is profusely illustrated by plates, many of which are coloured.

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*Contributions to the Physiology and Pathology of the Breast and its Lymphatic Glands.* By CHARLES CREIGHTON, M.D. London: Lewis. 1886. Pp. 222.

THIS work is mainly a reprint of papers which were published in the Reports of the Medical Officer of the Privy Council, 1884, 1885, and 1886; also of papers which were published in the *Journal of Anatomy and Physiology*, 1876 and 1879.

It is divided into two parts—physiological and pathological.

We feel that it would be impossible for us in the space at our disposal to give at all an adequate analysis of the author's observations and reasoning; and, while we differ completely from him in his conclusions, we admit most fully the painstaking and conscientious nature of his work and the ingenuity and suggestiveness of his speculations.

The processes which take place in the epithelial cells of the acini of the breast are described as those of an endogenous cell formation—this term being, however, used in a somewhat unusual sense, to include not only the formation of what we commonly call cells, but also that of a fluid substance. The process of development of function which the gland undergoes during pregnancy is divided into stages. The first of these is characterised by the formation of large cells, which contain yellow pigment. In the further stages we have "nuclear cells," then vacuolated cells, and finally only a fluid substance formed. When the gland is in full activity this fluid material, or milk, is discharged in the process of lactation, but when lactation ceases the gland suffers involution or goes back through the same stages as those which characterised its evolution, only in the inverse order. The formative activity of the cells does not cease immediately the functional activity of the gland terminates, but large numbers of cells continue to be formed, and constitute a waste product. These pass through the basement membrane of the acini, get into the lymphatics, and are carried to the lymphatic glands,

where they undergo various changes, and are finally modelled into white blood-corpuscles, in which form they reach the circulation.

As to the development of the mammary glands, the author rejects the usually received view that they are ingrowths from the rete mucosum of the epidermis, like sweat-glands, hairs, &c. His conclusions on this subject are:—

“1. That the mammary acini of the guinea-pig develop at many separate points in a matrix-tissue; that the embryonic cells, from which they develop, are of the same kind that give origin to the surrounding fat-tissue; and that the process of development of the mammary acini is step for step the same as that of the fat lobules.

“2. That the ducts of the mamma develop from the same matrix-tissue by direct aggregation of the embryonic cells along predetermined lines; that the ducts develop in the individual guinea-pig before the acini, whereas in the phylogenetic succession the ducts are a later acquisition; and that this reversal of the order of acquisition of parts is in accordance with the principle stated by Mr. Herbert Spencer that, under certain circumstances, the *direct* mode of development tends to be substituted for the *indirect*.”

In the study of the pathology of the breast, which is mainly that of tumour formation, the excess of formative activity over functional activity is made to play a chief part. The cells unduly formed accumulate in the acini, pass into the surrounding connective tissue and into the lymphatic glands and other parts, and then either by their overgrowth, or by exciting through infection the cells of the parts in which they lodge to abnormal development, they give rise to the different tumours of which the breast is so commonly the seat. The observations on which these conclusions are founded were made mainly on the diseased breasts of bitches and cats.

Although we believe the author is mistaken in his views, we think his work is one deserving of most careful consideration. No one who reads his pages can fail to be interested in the novelty and ingenuity of his arguments and deductions, or can avoid admiring the clear and pointed style in which his views are expressed.

The text is illustrated with well-executed engravings and one plate. The figures, however, in most cases, show only the nuclei, and the outlines of the cells are not indicated. We should imagine that the preparations from which the drawings were made were all stained in logwood and mounted in Canada balsam—a method by which, in many cases, much of the finer detail of the tissues is lost.

*The Diseases of Sedentary and Advanced Life: a Work for Medical and Lay Readers.* By J. MILNER FOTHERGILL, M.D.; Physician to the City of London Hospital for Diseases of the Chest (Victoria-park); late Assistant-Physician to the West London Hospital; Hon. M.D. Rush College, Chicago; Foreign Associate Fellow of the Royal [*sic*] College of Physicians of Philadelphia. London: Baillière, Tindall, and Cox. 1885. Pp. 296.

THIS book will be useful if it reaches the classes of readers for whom it is obviously intended; but it is possible that these may be deterred by its claim to be addressed to medical men as well as to unprofessional persons. In spite of the assertions on the title-page and in the preface, we cannot regard Dr. Fothergill's most recent production as a contribution to what is generally called "medical literature;" and if "the work is not written for" lay readers, "but rather for older medical men who have dropped behind in their reading, and younger men who, on encountering the actual difficulties of practice, find their hospital training not quite a complete equipment," it had better not have been written at all. To lay readers we recommend it; they will find in it much useful sanitary information, and many valuable hints. The physician, old or young, will find little or nothing in these three hundred pages to repay perusal. The author's style is not attractive—a stern critic might call it even slipshod—and showers of notes of admiration, falling where nothing admirable can be found, irritate the reader. Binders and printers have done their work well, and there are very few typographical errors.

Dr. Fothergill's remarks on the prognostic significance of cardiac murmurs (p. 225) are good, and may serve to remind some physicians, especially in examinations for assurance, of facts occasionally overlooked. It is sometimes forgotten both that murmurs do not necessarily indicate valvular lesion, and that "heart disease" does not necessarily shorten life. In Chap. XXXI. a subject is treated of which does not always, we fear, receive as much attention as it deserves from practitioners. Obesity is a cause of very real distress to many minds and bodies, and even medical men are apt to allow the ludicrous aspect of what Mr. Banting called "the parasite" to prevail, and to pooh-pooh complaints of ever-growing girth. Dr. Fothergill points out the danger besetting "Bantingism"—"the excess of albuminoids overruns the capacity of the liver to deal with it, and then follows in its wake renal disease." He quotes with

approval the following passage from the writings of the latest authority on the subject of reduction of obesity, Professor Ebstein, of Göttingen:—

“The permission to enjoy certain succulent things, always, of course, in moderation, as, for instance, salmon, *pâté de foie gras*, and such-like delicacies, reconciles the corpulent gourmet to his other sacrifices. These consist in the exclusion of the carbo-hydrates. Sugars, sweets of all kinds, potatoes in every form, I forbid unconditionally. The quantity of bread is limited at most to from three to three and a half ounces a day, and of vegetables I allow asparagus, spinach, the various kinds of cabbage, and especially the legumes, whose value as conveyers of albumen, as Voit observes, is known to few. Of meats I exclude none, and the fat in the flesh I do not wish to be avoided, but, on the contrary, sought after. I permit bacon fat, fat roast pork and mutton, kidney fat, and when no other fat is at hand, I recommend marrow to be added to the soups. I allow the sauces as well as the vegetables to be made juicy, as did Hippocrates, only for his sesam oil I substitute butter.”

From the author's own work we shall quote, in conclusion, his illustrations of “senility,” in which much feeling is displayed:—

“When a number of senile persons act in concert . . . the mental moods come out plainly, as is seen in the facility with which a good-looking young matron will turn the committee of an institution round her finger—provided that committee consist exclusively of grey or white-headed men. It is exemplified in self-elected oligarchies of senescent persons, as the Royal College of Physicians of London, for instance—which is little removed from an intellectual mummy swathed in rags and cerate. It is often markedly present in the chairman of a board of guardians or chairman of quarter sessions.” (P. 254.)

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*Micro-Organisms and Disease.* By E. KLEIN, M.D., F.R.S.;  
Lecturer on General Anatomy and Physiology in the Medical  
School of St. Bartholomew's Hospital, London. Third Edition.  
London: Macmillan and Co. 1886. 8vo. Pp. 267.

WHEN noticing the first edition of this excellent work shortly after its publication in 1884 (Vol. LXXVIII., No. 156, page 511), we commented on the diminutive size of the book and the smallness and closeness of the type used in printing it. Both these defects have been remedied in the new edition, which it is our privilege to recommend to our readers on the present occasion. It is much enlarged, and is printed in beautifully clear type.



We have already given a full account of the leading features of Dr. Klein's book, so that it will suffice to say that the author has made many alterations and additions in this—the third—issue. Some of the methods of preparing nutritive-media have been simplified, while others have been described in fuller detail. The methods of staining have been revised, and a number of facts bearing on septic and specific organisms, which have come to light within the past two years, have been added. At page 12, Lustgarten's, de Giacomi's, and Gottstein's methods of staining for the demonstration of the syphilis-bacilli are described.

The engravings now number one hundred and twenty-one, compared with one hundred and eight in the first edition. Among the new illustrations are those of a hot-water filter for filtering nutritive gelatine, or Agar-Agar mixture (page 30); a steamer for sterilising culture-material contained in test-tubes (page 31), plate-cultivation (page 43), cultivation of "foot and mouth" micrococcus on nutritive gelatine, after several weeks' growth (page 85); plate cultivations in nutritive gelatine, after three days' growth at 20° C., seen with the unaided eye (page 183); and a series of drawings illustrating the cultivation of the choleraic comma-bacilli (pages 175, *et seq.*).

We know of no book on the subject which is so easily understood, so interesting, and, we may add, so reliable as this by Dr. Klein.

*The Irish Medical Directory for the year 1886.* Dublin: Offices of the *Medical Press and Circular*. 1886. 8vo. Pp. 647.

THREE years have elapsed since we last noticed this annual publication, and on that occasion we drew attention to the presence of numerous errors and misprints, which could not fail to militate against the popularity and financial success of the work.

We regret to say that our strictures apply with even greater force to the present issue, which is more carelessly edited than any of its predecessors. The mere printers' errors in the book would be alone sufficient to condemn it. Such slovenly reading for the press it is seldom our misfortune to criticise. Here are a few specimens of misprints or misspellings of the names of places such as meet the eye on page after page:—Sleivareagh, Conaught, Broy, co. Wicklow; Richill, co. Dublia, Pomerey, eo. Dublin, Belmullett, Borrisokene, Ormean-terrace, Belfast; Nerthumberland-road,

Dublin; Beleek, Croggan (savours of Wales), Ballytare, co. Kildare; Killbeggan, Up. Fitzwilliam-street, Dublin; Mountjoy-square, Dublin; Balinrobe, Dublin, Balbriggan, Londonderry, Belfast, and so on to the end of the chapter. Then there are such words as "synæcologist," "Ledwieh," "contab" (as a contraction for "constabulary"), "ophthalmic," "Æsophagus," "Hæmorrhage," "auxilliary," and so on.

But these, after all, are minor faults. What shall we say of the complete omission of names of registered medical practitioners, of which we could quote many instances—and, worse still, of the gazetting in the Obituary List of the names of *several* gentlemen who happily are alive and well? Amongst the slain we find, for example, the name of our respected friend, Dr. John Eustace.

Again, the entries of some names are perfectly useless from their brevity and incompleteness—*e.g.*, "Appleby,—*Templemore-st., Belfast*;" "Meaney,—, *Clonee, Dungarvan*;" "Mason, J. Boyle, *co. Roscommon*," &c. This last stands, no doubt, for "Mason, J., *Boyle, Co. Roscommon*."

Under the heading "Distinguished and titled members of the Medical Profession and Parliamentary Representatives," we find the following among other items of information:—"Henry, Mitchell (M.P. for Blockfriars district of Glasgow)," &c. Mr. J. E. Kenny, M.P. for South Cork, is stated to have "received the *degree* of L.A.H. Dublin in 1868 from the Catholic University"! This absurd *olla-podrida* seems to have been copied *verbatim* from *Thom's Directory* for the present year. Mr. Kevin Izod O'Doherty, M.P. for North Meath, is described as "son of — O'Doherty, of Dublin; born in 1823, and educated for the Medical Profession, but before he had graduated he, in 1848, after the suppression of the *United Irishman*, took an active part in starting the *Irish Tribune*. He was arrested and sentenced to ten years' transportation to Van Dieman's Land." (What a demoniacal proceeding!) "He was subsequently granted a pardon, with the condition that he should not reside within the United Kingdom." In fairness to this gentleman, we feel bound to give his subsequent history, which is very much to his credit—"He then renewed his studies in Paris, and in 1856, having been granted a free pardon, he returned to Dublin, and was admitted a Fellow of the Royal College of Surgeons, Ireland, in 1857, and Licentiate of the King and Queen's College of Physicians, Ireland, in 1859. After practising for some years in Dublin, he emigrated to Queensland, and practised as a physician in Brisbane. He became a member of the

Legislative Assembly, and afterwards of the Legislative Council. He has only recently returned to Ireland."

Sir Lyon Playfair—we are told—studied at Giessen, "under B9ron Liebig," and sat for the Universities of Edinburgh and St. Andrews from "*Decemeeer*, 1868, to 1885."

Passing to what (in our opinion) are the most original and most useful parts of the book—the sections dealing with Poor-law and Registration Regulations and with the Acts of Parliament specially affecting the medical profession in Ireland, we find their value seriously impaired by wretched type and frequent typographical errors. Small "b" takes the place of "h," "a" of "n," and "d" of "p" in the most impartial manner; while "s" and "n" are so often inverted that these unhappy letters evidently do not know whether they are standing on their heads or their heels.

Under the heading "The Medical Schools and Hospitals of Dublin," the School of the Royal College of Surgeons naturally occupies in this work the foremost place. We are informed that it "is attached by Charter, and has existed as a department of the College for nearly a century." Among the Professorial Chairs we find the following which are new to us:—"Phytology," "Anasemy," "Gynæcoogy," and "Practical Anatomg." Our friend, the distinguished King's Professor of Institutes of Medicine in the School of Physic in Ireland, will be surprised to find himself described (on page 411) as "Midwifery Physician" to Sir Patrick Dun's Hospital, of which institution also Dr. Kirkpatrick figures as one of the "*Clinical Surgeons*."

At page 473 a list of drugs and medicines, their doses and prices, is given. But it is evidently quite out of date, for it contains the omissions from the British Pharmacopœia of 1885, and makes no mention of most of the additions to that work.

There is one portion of the "Directory," in which accuracy was especially to be expected—namely, the advertisement department; but even here an evil genius seems to have dogged the editor's footsteps—or, rather his pen-point. The name of a much-lamented member of the profession who died early last autumn still appears on the staff of the House of Industry Hospitals. Dr. W. J. Smyly is described as "Gyæncologist" to the City of Dublin Hospital, "Examiner in Midwifery, Royal College of Surgeons," and "Lecturer in Midwifery, Carmichael College of Medicine." To the same hospital "Mr. Joliffe (*sic*) F. (*sic*) Tuffnell" (*sic*) still appears as Consulting Surgeon. The senior physician to the Meath

Hospital is stated to be "Lecturer on Practice of Medicine, Ledwich School of Medicine"—an appointment vacated by Dr. Foot three years ago, when he was elected Professor of Medicine in the School of the Royal College of Surgeons in Ireland. Dr. J. W. Moore is presented with a non-descript qualification, which does not exist—namely, "M.R.C.S.I. Univ. Dub.;" Mr. Wharton is re-christened "John," and Mr. Smyly is made to spell his name "Phillip." In the advertisement of the "Institution for the Treatment of Diseases of Children, Pitt-street," is found the honoured name of one who passed from this earthly scene more than eighteen months ago; and the advertisement of the National Orthopædic and Children's Hospital bears internal evidence in four particulars of being at least three years out of date—the "Vice-Patron" died two years ago, and the "Physician" has been an Inspector of the Local Government Board, and has resided in Belfast even longer than that time.

In concluding this unfavourable notice, we have only to say that the task of writing it has been as unpleasant as it well could be; but year after year we have watched with dismay a steady decline in the literary merit of a publication which perhaps never possessed an adequate or a satisfactory *raison d'être*.

*The Laws and Mechanisms of Circulation, with the Principles Involved in Animal Movement.* By WM. H. TRIPLETT, M.D.  
New York: J. H. Vail & Co. 8vo. Pp. 510.

PERHAPS we can best give an idea of the style and pretensions of this book by reproducing the first paragraphs of the Preface:—

"In the present work, fresh ground is broken and a new outlook afforded biological and physiological science, by means of which the domain in living structure is easily surveyed, and the multitudinous phenomena, anatomical and physiological, hitherto inexplicable, readily accounted for. And the prodigious labors of self-denying and painstaking students of anatomy, natural history, biology and physiology, in past and present time, may be utilised in a systematic work, in which all the parts show continuity of relation and interdependence, the same as in the living organism which it portrays; at the same time, incalculable benefits, psychical as well as physical, must inevitably flow from it. A grand forward movement, in short, placing physiology abreast of the physical sciences, and dealing with the problem of problems, or that concerning life, by far the most interesting. We congratulate mankind

upon the consummation of the long-expected and devoutly wished-for event.

"The method adopted is easy and natural. It consists in seeking out the adjustments in the living organism with the forces in nature, notably, pressure and gravitation; the one acting in *all* directions, the other in *one* only, or towards the earth; this, together with the *principle* involved in animal movement (which is fully shown), gives the solution in animal structure and function. Such, in brief, is the course adopted in the text, which furnishes incontrovertible evidence to the correctness of the premises, making the argument unanswerable."

To those who would relish six hundred pages of this sort of matter, expressed in this sort of style, we can recommend "The Laws and Mechanisms of Circulation," but we are afraid there are many of our readers who, like ourselves, fail to see the great happiness which has fallen on mankind through the labours of Dr. Triplett, and who feel that life is too short to justify the expenditure of the time that it would cost to extract the very small grain of wheat which is concealed in the huge mass of chaff of which this volume is mainly composed.

*Selected Monographs.* London: The New Sydenham Society. 1884.

THE subscribers to the New Sydenham Society will not, we think, find fault with the contents of this very interesting volume, which includes essays upon three widely different subjects.

1. A valuable monograph, by Dr. H. Senator, upon Albuminuria in Health and Disease, with two appendices—(a) a contribution to the theory of urinary secretion; (b) the hygienic treatment of albuminuria.

The translation of this essay is due to Dr. T. P. Smith; and a separate index to it is fortunately supplied.

2. Dr. Cayley edits a paper by the late Dr. A. P. Stewart, On the Nature and Pathology of Typhus and Typhoid Fever. This paper was read before the Parisian Medical Society in April, 1840; and to Dr. Stewart belongs the credit of being one of the first to see clearly the essential distinction between typhus and typhoid fever, and to have supported it by full and convincing arguments. The editor contributes a short and appreciative memoir of Dr. Stewart.

3. Dr. F. H. Champneys translates and edits with care an important monograph by Dr. L. Landau on Movable Kidney in

**Women.** The scientific and clinical value of this treatise is considerable. It constitutes a welcome addition to our knowledge, corrects mistaken views in anatomy and pathology, and deserves to be studied by every medical man. It is especially commended to the notice of gynæcologists, inasmuch as the subject stands in a more intimate relation to affections of the female sexual organs than is commonly supposed. A separate index is provided.

The essay on albuminuria is a full and practical account of a matter of deep interest to every physician. The principal topics discussed are—the various forms in which albumen is excreted; albumen as a constituent of normal urine; the dependence of albuminuria upon alterations in the blood-pressure, upon degeneration of the renal epithelium, and upon the condition of the blood; and, lastly, certain peculiar forms of morbid albuminuria.

Although we are still far from a complete solution of many of the difficult problems involved, Dr. Senator's monograph is a masterly contribution, and sheds a critical light over some of the dark places in the pathology of albuminuria.

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*The Pocket Formulary and Synopsis of the British and Foreign Pharmacopæias.* By HENRY BEASLEY. London: J. & A. Churchill. 1886. Pp. 517.

THIS little book—now in its eleventh edition—contains a very large amount of information which will be found useful not only by those who compound, but also by those who prescribe drugs. We have here arranged in alphabetical order the modes of preparation and doses of a very large number of medicines, including, in addition to the drugs and mixtures which are employed in this country, those which are in use in America and on the Continent. A large number of hospital and other formulæ are also introduced. In the appendices we have some useful tables of comparison of the English weights and measures with those of the metric system; a table regulating the proportion of doses according to age; and a list of English, French, and Latin synonyms. As a book for reference it will be found useful, especially by those who come into contact with foreign medicines and modes of prescribing.

## PART III.

### MEDICAL MISCELLANY.

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*Reports, Transactions, and Scientific Intelligence.*

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#### ACADEMY OF MEDICINE IN IRELAND.

President—ROBERT M'DONNELL, M.D., F.R.S.

General Secretary—W. THOMSON, M.D.

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#### PATHOLOGICAL SECTION.

President—T. EVELYN LITTLE, M.D.

Sectional Secretary—J. B. STORY, F.R.C.S.I.

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*Friday, February 12th, 1886.*

The PRESIDENT in the Chair.

#### *Villous Disease of Kidney and Rupture of Heart.*

DR. THORNLEY STOKER exhibited the kidneys and heart removed from the body of a gentleman who had recently died at about the age of seventy-two years. For ten years preceding his death he had suffered from occasional attacks of renal colic and hæmaturia. The loss of blood at times had been enormous, and the length of the hæmorrhages had varied from a day or two up to ten or twelve days. Great difference of opinion had existed as to the nature of his disease—some of the numerous gentlemen he had consulted having considered the kidneys as its seat, while others had referred it to the bladder. Villous disease of the bladder and renal calculus were among the conditions said to exist.

A *post mortem* examination showed that the immediate cause of death was rupture of the left ventricle of the heart, due to fatty degeneration and hæmorrhagic infarction of its anterior wall, the result of embolism of the anterior coronary artery.

This and other diseased conditions of the vascular system were evidently secondary to kidney disease. The left kidney was the seat of chronic nephritis, and its sinus contained two oxalate of calcium calculi as large as hazel nuts.

The right kidney weighed 1 lb. 7 oz. ; its substance had almost disappeared, and was replaced by a number of spaces filled with blood-clots in different stages of formation—some being old and well formed, while others were recent and scarcely solidified. The pelvis of the ureter was filled with thin stirabout-like matter, which proved to be disintegrated blood. The specimen had then been carefully reported on by Dr. Alex. M'Kee, who discovered in the sinus, infundibula, and calyces of the kidney extensive villous growths, presenting the usual dendritic character. The theory put forward by Dr. M'Kee was no doubt correct—viz., that successive hæmorrhages dependent on the villous disease had, by the deposition of successive layers of coagula, formed the laminated and spheroidal clots which had by pressure dilated the normal passages of the organ and destroyed the kidney substance.

Dr. Thornley Stoker stated that he had no doubt as to the correctness of Dr. M'Kee's theory, and that inasmuch as the renal pain complained of during life was sometimes felt on the right side and sometimes on the left, it was doubtless due in the latter case to the passage of calculi similar in structure to those found in the left kidney after death ; and in the former case was due to the passage of clots. The latter fact was further established by the knowledge that while the patient frequently had pain without hæmaturia, he never had hæmaturia without pain ; and that a discharge of stirabout-like matter composed of disintegrated blood, such as was found in the right ureter after death, was often the immediate precursor of relief from pain.

That report appeared to him (Dr. Stoker) to be perfectly conclusive, and quite in accord with what they knew of matters bearing on diseases of the kind. The right kidney was exceedingly well worthy of examination. The condition of villous disease which it exhibited was, he believed, undescribed—at least he had not been able to find a record of anything like it. They were aware that such disease occurred in the bladder, and even in the gall bladder. The question arose, whether the attacks of renal colic from which he suffered arose from the hæmorrhages and the diseases in the right kidney, or from the calculi in the left kidney, inasmuch as the pain he complained of occurred sometimes in the right loin and sometimes in the left. Possibly the renal colic resulted from both the calculi and the passage of the blood-clots. That he did suffer pain from the passage of the blood-clots was conclusively proved, not only by the occasional sites of the pain on the right side, but also by the fact that on the occasion of one of his most severe attacks of colic he described the thin-stirabout-like matter as coming away, and said that when it did come away he got relief from the pain.

DR. JAMES LITTLE said he had watched the gentleman for the last ten years, although he had not seen him for eighteen months before his death. He was very well known in Dublin. He belonged to the tem-



perament which was usually described as sanguine, had rather a florid face, and was very active in mind, and also in the movements of his body, as he was generally met hurrying through the streets by those who encountered him. He had been during ten years subject to the attacks of hæmorrhage. What usually happened was, that when going home in the afternoon from his office he would begin to feel pain, which usually came in the left loin, but sometimes in the right. He used to get into bed as quickly as possible, and the pain usually got worse and worse, and went down towards the groin and into the testicles, and had all the characters—as he was informed, for he never saw him while suffering from one of those attacks—of renal colic. His son, who was a medical man and lived with him, was in the habit of administering morphia, hypodermically and otherwise, but it never relieved him unless it was administered in such quantities as to stupefy him. The only thing that relieved him was a very hot application to his loins. The pain usually lasted twelve hours, and sometimes longer. Whenever he had pain he always had hæmaturia, but occasionally he had hæmaturia without pain. He (Dr. Little) several times examined his urine, both immediately after it was voided and after intervals of days, but with the exception of blood cells and small octahedra of oxalate of calcium, he never found any microscopic objects in it. Generally after an attack of hæmaturia the blood would, after a couple of hours, disappear from the urine, and it would become of a pale sherry colour. Sometimes the blood disappeared abruptly from it. From the entire absence of any vesical symptoms he always believed that the hæmorrhage came from the kidneys; and the only guess he could make as to the cause of the pain was that a clot of considerable size had got caught in one of the ureters and gave rise to pain as a small calculus would. He never was solicitous about stopping the bleeding, for it always appeared to him that he was a good deal better in his general health after the bleeding. The only occasion on which he ever saw him at his own house was some years ago, and on that occasion he was in a semi-comatose condition from cerebral congestion, which was relieved by the withdrawal of blood from his head. He therefore concluded that he made blood with great rapidity, and that the loss of blood he sustained during the attack did him no harm. Since he saw him last he heard that he had got into a very anæmic state from loss of blood. The diagnosis made was that the blood came from the kidneys, and his idea was that the calculi in the kidney occasionally irritated the pelvis and gave rise to the hæmorrhage. But that did not account for the changes in the right kidney.

DR. J. W. MOORE said the present case proved that renal colic might be produced by the passage of blood-clots. An instance of this recently came under his observation in the Meath Hospital. A young man, aged about thirty or thirty-two, was admitted passing large quantities of

bloody urine. After some days' treatment the blood began to lessen, and then for the first time he began to complain of very acute pain—which from time to time became paroxysmal—in the left side of the abdomen. On examining his urine he was surprised to find that instead of the blood being diffused through it there were distinct blood-clots in the urine; and he came to the conclusion that the pains were produced by the passage of these clots, which afterwards became lodged in the pelvis and clotted there also. Another point of great interest was illustrated by a case which he saw a year and a half ago. It was that of a gentleman of advanced age who had for many years suffered from gout in his system, and who had been in the habit of passing quantities of oxalate of calcium in his urine. He was of active habits, and was suddenly laid up with an attack of pulmonary congestion. His convalescence from this was protracted, and he had to give up all active exercise. He was a good horseman, and had been in the habit of taking daily exercise on horseback. When his convalescence was fully established he went out riding one day, and came home with a great desire to micturate. On doing so he was horrified to find that what he passed was almost pure blood. He sought advice, but in a very few hours his urine became perfectly limpid and pure. He (Dr. Moore) examined the bloody and also the pure urine, and found both to contain large quantities of calcium oxalate, the microscope showing numerous very minute calculi. His reading of that attack was that during his enforced inactivity the gentleman had accumulated large quantities of calcium oxalate in the pelvis of the kidneys, and that when he rode on horseback these microscopic calculi were disturbed, and passing down the ureters irritated the mucous membrane and caused the sudden hæmorrhage. He made an admirable recovery, and was not troubled with bloody urine afterwards. There was no pain in his case, because the calculi were not large. The gentleman was sixty-nine years of age.

DR. DUFFEY thought they should look for something more than fatty degeneration in order to account for the rupture of the heart. He did not hear whether Dr. Stoker had said that there was fatty degeneration of the muscle of that organ; but if fatty degeneration, *per se*, had been the cause of this rupture, then they should meet with ruptures of the heart more frequently. Dr. Hayden had reported ninety-five cases of fatty degeneration of the heart, in only seventeen of which there were ruptures. Dr. Stoker had explained that in his case there was more than fatty degeneration—namely, calcification of the arteries and hæmorrhagic infarction. In a case which he (Dr. Duffey) showed at the Pathological Society five years ago there was well-marked atheromatous plugging of the coronary arteries and rupture of the right auricle. In Dr. Stoker's case the rupture had occurred in the anterior wall of the left ventricle. Another point of interest in his case was the syncope which he said

occurred the day before death. It struck him (Dr. Duffey) that perhaps the rupture might have occurred on that day, for it was established that rupture might occur and the fatal termination not take place for several hours afterwards. If death had occurred immediately after the rupture the blood would not have been so firmly clotted as Dr. Stoker had stated.

DR. HENRY KENNEDY said that in cases of this kind hæmorrhages were commonly quite independent of both the kidneys and the heart. He had seen a number of cases of persons subject to fatty degeneration, in which fat existed in large quantities all over the body, and the individuals were subject to terrific fits of epistaxis, and yet afterwards made perfect recoveries for the time being. He looked on it as almost a necessary condition in those cases that hæmorrhages would occur independently of the state of the kidneys. He had not heard anything as to whether this gentleman was subject to gout or not; but he thought that he must have had the gouty poison in him, and that it was relieved from express determination to the feet or elsewhere by these hæmorrhages. He had himself seen cases in which hæmorrhages warded off regular attacks of gout. Persons of the temperament described, in which blood was made very rapidly, were very subject to hæmorrhages from different parts of the body, the bowels, and the nose. He had not heard Dr. Stoker say whether there was any fatty degeneration of the heart in his case or not.

DR. STOKER, in reply, said he had long held the opinion—and he did not remember who taught it to him, for it was not original—that men as well as women were subject to a change, the result of advancing years, but which came much later than with women—namely, at sixty-five or seventy years of age. He had occasionally been consulted by elderly gentlemen who suffered from profuse urinary hæmorrhage, which got well after a time, and for which he had been unable to find a cause. He remembered a case ten years ago of a gentleman living in Rathgar, who was then nearly seventy years of age, and who nearly bled to death from hæmaturia, although he had no organic disease at all. He looked on these hæmorrhages as depending on a male change of life, and he was rather inclined to think that Dr. Little's case belonged to the same class. As to Dr. Duffey's question, he did state that the heart in his case was in a state of fatty degeneration. They knew that in the bulk of the cases of rupture of the heart fatty degeneration existed; but there were some in which there was no fatty degeneration, and hæmorrhagic infarction had been found to be the immediate cause of the rupture. As to the time at which the rupture occurred, there was no doubt that it might be a very gradual progress. The President had contributed a case which bore very strongly on this point. No doubt in many cases the rupture began at the surface of the heart, where the convexity was greatest, just as a breakage occurred in the hoop of a barrel; but it was

equally true that the rupture might begin in the substance of the wall of the heart, or even in its interior. He believed that the symptoms which manifested themselves on the night preceding the death of his patient were due to a partial rupture having taken place, and that at the moment preceding the patient's death the rupture was completed, and the hæmorrhage into the pericardium was the accompaniment of death; for he had no doubt that the coagulum which occupied the pericardium was of *post mortem* formation.

*Cancer of the Liver.*

DR. JAMES LITTLE exhibited and discussed a specimen of cancer of the liver. He said it was worth notice—first, because it was rare; and, secondly, because a complete though pardonable mistake had been made in the diagnosis of it. On the 1st of January, 1886, a lady's nursetender, aged forty-four years, was brought to the Adelaide Hospital suffering from profuse hæmoptysis. She had been attending a lady in Molesworth-street and went to bed with a new-born infant. Soon afterwards she fell sick and vomited, as she said, half a chamberful of blood. From the quantity she brought up after her admission into hospital it was probable that that was not an exaggeration. On seeing her he inquired into her previous history and found that she had been always a delicate woman, and during the past year particularly so, the chief ailment from which she suffered having been pain in the epigastrium, brought on by eating solid food. She mentioned three or four physicians whom she had consulted. The pain went between her shoulders, and particularly under the right shoulder blade; and it was always brought on by eating solid food, but was occasionally relieved when she took liquid food. She had also suffered from nausea, particularly in the morning, and had sometimes vomited, but had never vomited blood until the occasion mentioned. His diagnosis was that she had an ulcer of the stomach. She mentioned that whenever she took aperient medicine it greatly aggravated the pain; and also that when her bowels were about being moved, and also for some time afterwards, she suffered from a great feeling of soreness all through the abdomen. The existence for twelve months of the epigastric and interscapular pains, and the circumstances of their having grown worse during the last three months, and of their having been particularly bad on the day preceding the hæmorrhage, seemed to him sufficient to justify the diagnosis of an ulcer in the stomach. Another circumstance tending to the same conclusion was that the blood was very bright in colour, and not of the dark colour that usually came from the stomach. She lived only twenty-four hours after her admission, and had several profuse hæmorrhages before her death. A *post mortem* examination was made by Dr. Bewley, who had given the following report of it:—

"On *post mortem* examination the stomach was found to be quite healthy, with the exception of a patch near the lesser curvature, about the size of half-a-crown, where the veins were greatly dilated and varicose. There did not, however, appear to be any breach of surface.

"The intestines contained a large quantity of blackened blood, but their walls appeared healthy; the œsophagus was quite normal, and no breach of surface in any part of the gastro-intestinal tract could be found. The liver weighed 4 lbs.; almost the whole of the right lobe was occupied by one large mass, which presented in places a white and in places a yellowish-brown colour, and which was very firm and dense, cutting almost like cartilage. This mass was bounded by an abrupt line of demarcation from the rest of the liver. The remainder of the liver, consisting mainly of the left lobe, was very tough, and presented the characters of cirrhosis. The vena portæ was filled with a soft dark thrombus.

"On microscopic examination the mass in the right lobe of the liver was found to be cancerous in nature, consisting of loculi of firm fibrous tissue, completely filled with polygonal-shaped cells. At its margin this mass pressed on the liver cells, causing them to atrophy and disappear. Many of the branches of the portal vein are filled and rendered impervious by a growth of the cancer in them. The left lobe of the liver shows the microscopical characters of cirrhosis. In both lungs were several nodules, about the size of a small cherry, which have exactly the same structure as the mass in the liver. All the other organs in the body appeared healthy, as far as they could be examined.

"The course of events in this case seems to me to have been as follows:—The cancer in the liver grew into the branches of the portal vein, gradually obstructing more and more the blood-flow through them. At last the blood, not being able to circulate, clotted in some branch, and from this the thrombosis spread through the portal veins in the liver, thus completely, or almost completely, checking the flow of blood in the portal vein. This caused so great congestion of the stomach and intestines that their over-full capillaries poured out the blood in the enormous quantities which were vomited.

"That the abdominal viscera were greatly congested is shown by the fact that, in spite of the great loss of blood, the spleen was found, after death, to be very full of blood, and to weigh 10 oz."

The case then was one of cancer of the liver occurring under unusual circumstances, and not presenting the characters of ordinary tubercle of the liver. It was a mass of hard cancer occurring in a liver which had been previously the seat of cirrhosis, and the extensive hæmorrhage was evidently due to sudden obstruction of the portal vein.

DR. MACSWINEY asked whether, during the patient's life, palpation or percussion in the region of the liver was tried, and whether increased size or any other abnormal appearance was detected?

DR. LITTLE, in reply, said he did not observe any outward signs of change in size of the liver.

*Spontaneous Rupture of the Heart.*

DR. FOOT communicated a case of spontaneous rupture of the heart. He said that his contribution to the subject of rupture of the heart derived any interest which it might present from the fact of the rupture having occurred in bed during sleep. In all other respects it accorded with the majority of reported cases—that is, it happened in a man, in one over sixty years of age; the heart was in a condition of fatty degeneration, and the rupture was in the left ventricle on its anterior aspect. Rupture of the heart—under no circumstances a very common event—is still less frequently met with independent of physical effort or mental excitement. The subject of the event was a car-driver, sixty-six years of age, admitted to hospital exactly a fortnight before his death complaining simply of weakness and short breathing. He said he had been ailing but for a week before admission. At the time he came under observation his feet and legs were œdematous, the action of the heart weak, slow, and unequal, his colour pale, and the skin of a satiny smoothness. His appetite was good, and urine free from albumen; and under the use of rest, meat, and wine, with tonic doses of digitalis and nux vomica, he frequently said he was daily gaining strength, but never showed any inclination to leave his bed. He slept a great deal, and often had to be aroused when the class came round. His sleep was never of a lethargic or comatose variety. On the morning of the 9th of February, 1885, he was found dead in his bed. There was no indication of any discharge from his stomach or bowels having taken place, nor was there any disturbance of his bed-clothes; his face was quite placid; he was such a quiet man, and usually slept so much that his not moving in the morning was unnoticed by the other patients; he had made no noise during the night. The distended pericardium, when slit open, gave exit to blood-stained serum, and contained 12 ozs. of jet black coagulum, part of which was connected with a longitudinal rent in the anterior surface of the left ventricle, midway between the apex and base and close to the septum ventriculorum.

The pale, soft, and greasy muscle of the heart had obviously been the seat of fatty degeneration, the condition which, above all others, predisposes to rupture. In 62 cases of rupture, Quain found it present in 25; and in the cases more recently collected by Barth, it was so in 19 out of 24.

I do not think the rupture can be attributed to the use of digitalis—a possibility which has been suggested by Lauder Brunton—the digitalis by the energy of the systole causing rupture of the degenerated fibrils—because the digitalis was given in small doses, was unattended by nausea, anuria, or marked slowing of the pulse, and he expressed himself as feeling an almost daily improvement under its use.

THE PRESIDENT remarked that a feature in this case was the very large size of the rupture; and its occurrence under the circumstances described, during sleep and without any muscular exertion, was rather unusual. Ruptures of the heart were generally longest at the inside, and passing obliquely outward towards the pericardial surface until they formed a pretty small rent at the outside; and this was an argument in favour of what Dr. Stoker had, in connexion with his case, indirectly put forward—namely, that rupture of the heart in many cases took place gradually.

DR. STOKER said he was struck by the great size of the rupture, and felt inclined to ask could it have been possible that any one put his finger into it? He had not stated that fatty degeneration was not a cause of rupture. Quain had lately published statistics of 100 cases of rupture of the heart, and in 66 of these there was fatty degeneration. The question he did raise was, what the nature of the fatty degeneration was—whether it was part of a general change in the system, or only occupied a circumscribed area. In the heart he showed, although there was a considerable amount of fatty degeneration all through the organ, there was an excessive amount at the place where the rupture occurred. In Dr. Foot's case it might be well to slit the coronary arteries, and see if they were occupied by thrombi.

DR. FOOT, in reply, said as to the size of the rupture it was possible that it might have been fingered at the meeting of the Biological Association, where the specimen was exhibited. These ruptures had been compared by some authors to the aperture a bullet made in leaving a living body. No doubt, in this case, the aperture was very large at the commencement. He did not slit the coronary arteries, but was satisfied that they were not calcified.

#### *Cardiac Disease.*

DR. WALTER SMITH exhibited the viscera taken from a man who died of cardiac disease. The patient, aged thirty-seven years, was admitted into Sir P. Dun's Hospital, January 15th, 1886, and died on the 21st of that month. He was a ship-chandler, and drank hard. Twenty-three years ago he suffered from rheumatic fever, but remained in good health up to two years ago, when he began to suffer from shortness of breath and pain over the heart. In December, 1885, he sought advice for cough, dyspnoea, pain in the chest, and insomnia. Upon examination the heart was found to be much enlarged, and several murmurs were heard; at the base of the heart, systolic and diastolic murmurs, and at the apex a localised systolic murmur, and a doubtful presystolic one. Pulse small and weak. Bronchial râles over right lung; dulness over back of left lung. Viscid muco-purulent sputum, tinged with blood. Feet swelled; no albumen in urine. He rapidly declined in strength, and died suddenly, January 21st.

*Post mortem.*—Right pleura normal; left much thickened. Both lungs in a state of marked brown induration. A considerable hæmorrhagic infarction in lower lobe of right lung. Heart weighed 17 ounces. Endocardium thickened and opaque in right and left cavities. Dilatation of right cavities. Pulmonary valves healthy. Walls of left cavities, especially ventricle, thickened, and very firm in texture. Mitral valve constricted, scarcely admitted little finger; warty growths and cauliflower excrescences, chiefly on its auricular aspect; curtains of valve adherent and blended into a cone-shaped funnel. Aortic orifice—warty vegetations on all the segments, which were thickened and adherent, commencing atheroma in aorta. The abdomen contained several pints of fluid; a large pale infarct in spleen; a small infarct in right kidney, and minute hæmorrhages in pelvis of each kidney; substance of kidneys firm and tough; liver equal to  $2\frac{1}{2}$  lbs., very hard and tough; nodulated on surface; section very dark and mottled. Walls of intestines thickened and œdematous; minute hæmorrhages scattered over the mucous membrane; stripes of ecchymosis along great curvature of stomach, and in lower end of œsophagus.

DR. MACSWINEY asked what was the actual condition of the heart as to hypertrophy or dilatation? Was there any evidence of the compensatory changes that usually took place in such cases; or evidence of regurgitation or insufficiency through the tricuspid valves denoted by pulsating jugulars?

DR. FINNY asked did Dr. Smith notice any alteration in the appearance of the lungs?

DR. SMITH, in reply, said there was no evidence of tricuspid regurgitation or alteration in the pulse while the man was under his observation. He could not reply to Dr. Finny's query, because he had made no microscopic examination; but he believed it had been found to be nearly universally the case that when the small vessels were greatly dilated the sections of them were figured in some of the text-books as like epithelial cells pigmented, whereas they were only capillaries stuffed with blood corpuscles.

The Section then adjourned.



## SURGICAL SECTION.

President—SIR CHARLES A. CAMERON, M.D., President of the Royal College of Surgeons, Ireland.

Sectional Secretary—WILLIAM STOKES, F.R.C.S.I.

*Friday, February 19, 1886.*

DR. ROBERT M'DONNELL, President of the Academy, in the Chair.

*Pharyngocoele.*

MR. WHEELER communicated a case he had successfully operated on for pharyngocoele and dilatation of the pharynx, together with a diverticulum at the lower portion of the pharynx.

The patient was aged fifty-seven years, had a fair antecedent history.

In 1884 he complained first of the above affection, which caused the following train of symptoms:—He could not lie down at night without feeling a sense of choking, as if his throat was impeded by some large mass; so great was his distress that he had to get out of bed and sit in a chair; in rising from the horizontal to the perpendicular he was subject to recurrent fits of coughing; during a night he would get up about a saucer-full of clear sticky fluid, with occasional froth; talking was always followed by coughing, and was very indistinct, resembling the voice of one whose vocal cords had been eroded; at times he was quite unintelligible. To swallow food was very difficult, except in small particles, and even then they stuck in his throat, to be again coughed up some time later.

When Captain E. first presented himself to Mr. Wheeler a tumour was observable on the right side of the neck, lying over the ramus of the lower jaw and extending to below the thyroid cartilage.

The patient was operated on the 13th of June last. An incision having been made from the ramus of the lower jaw to well below the thyroid cartilage, a large piece was cut out of the pharynx commencing a little below the ramus of the jaw to the lowest part of the dilatation. And now the pharynx had to be pulled up, as a pouch existed at its inferior portion, and lay posteriorly to the œsophagus; this pouch was about the size of a walnut, and in it the food used likewise to lodge. The edges of the pharynx were brought together by points of interrupted suture, afterwards the muscular tissue, and finally the skin.

Mr. Wheeler was unable to obtain a record of any similar case that had been operated on; and quoting from Von Ziemssen, he found that 27 autopsies had been recorded, that author stating "that the radical cure of diverticula from without is at the present time one of our vain wishes." In conclusion, Mr. Wheeler stated that the patient was in excellent health, and that his voice was quite restored. Photographs and drawings of the patient were exhibited.

DR. DUFFEY said Mr. Wheeler had, by the operation which he so graphically and accurately detailed, made a name as a pioneer in a new field of surgery. The causation of the diverticula he attributed to the straining use of the patient's voice; but a great many persons used the voice to the same degree without causing diverticula of the pharynx. There were other more rational explanations, as the stoppage of a foreign body in the mucous membrane of the pharynx. In twenty-two cases reported all occurred in men aged forty years and upwards. Mr. Wheeler's patient was aged fifty-seven. There were no reported cases occurring in women. He asked had there been an examination of the walls of the tumour for muscular fibres, the existence of which was a moot point, while their presence would negative the suggestion of hernia.

The PRESIDENT thought the probable cause of hernia or aneurysm of the pharynx was still in obscurity. Although Dr. Duffey's suggestion might seem a starting point, there must have been some force going on to make a tumour of the size described. The upper portion of the pharynx being completely open and no air pent up, it was difficult to determine what force produced the bulging.

SURGEON-MAJOR TOBIN inquired whether the persons whose cases were recorded had played wind instruments.

MR. WHEELER, in reply, believed there were no muscular fibres; but in the pharynx itself, dilated down to the next pouch, there were.

DR. DUFFEY.—I meant microscopically.

MR. WHEELER—I do not think there was any microscopical examination made; but the muscular fibres were quite plain to be taken off between the two pouches. There were none below the pouches, so far as I know. I am not aware that wind instruments are alluded to in the literature of the subject, which is very sparse. In pressure diverticula, the cause has been ascribed to pressure on the throat on account of the larynx being enlarged.

#### *Wood's Operation for Extroversion of the Bladder.*

DR. E. H. BENNETT submitted to the Section casts of the original deformity and of its modification, by Wood's operation, in a case of extroversion of the bladder.

The patient was a female on whom the first operation was performed, three years ago, when the child was a year old. Previous to the operation the urine was frequently observed to flow guttatum from the orifices of the ureters, the deformity being of the ordinary variety.

The first operation effected the complete covering of the exposed bladder surface, but as the wounds healed, a considerable shrinking of the flaps took place and the orifices of the ureters were again exposed. After some months the urinary flow was seen to take place in irregular squirts, not guttatum, a condition which could only be explained by

assuming the existence of dilatation of the ureters. Two operations which consisted in the paring of the edges of the flaps, converging in the middle line, served to close the opening in which the vesical membrane was exposed, as far down as the cleft clitoris.

Nothing now remained but to adjust an apparatus for conducting the urine into a proper receptacle. Attempts in this direction were commenced in June, 1885, when the child was four years old, and when an interval of many months had elapsed after the last operation. At this time the child did not appear in as good health as previously, and this failure increased slowly until her illness became extreme last month. Her death was brought about by constant vomiting and wasting.

The examination of the pelvic viscera and site of the operation proved that the cause of death was purulent inflammation of the ureters and pelves of the kidneys, with great dilatation of the ureters. The death appeared, therefore, to be due to gradual contraction of the sites of the various wounds, which had strained the orifices of the ureters, so as to cause a permanent obstruction of their excretion. The observation of the change in character of their flow after the completion of the first operation, and while the openings could still be watched, shows that the contraction was due to the first operation.

MR. W. THORNLEY STOKER considered, apart from the case being an unusual one in operative experience, that the point of interest was the fatal result and the warning which it afforded. As to the difficulty of determining the sex, he had a case in point. Within the last month a mother and child from the West of Ireland were admitted into the Richmond Hospital in order that the child, then aged two months, might receive surgical treatment for a double inguinal rupture of the congenital variety. He asked what was the child's name. The mother said, "Mary Anne." Then he asked had the child been baptised. With some indignation the mother answered, "Of course, the child had been both christened and registered." "Well, ma'am," he rejoined, "Mary Anne is a boy!" On examination, Mary Anne had a very large penis and testicles. The sex had been disguised by a double rupture passing downwards and inwards, giving the appearance of two enormous labiæ, and by a very slight hypospadias, the urethra opening half an inch behind the glands. On reducing the ruptures and pushing back the sacs which contained them, the penis became very apparent.

MR. WHEELER said he had two cases eight years ago, which were of interest on the subject—one of external eversion of the bladder, and the other of ectopia vesicæ with fissure. The external eversion of the bladder occurred in a young woman, aged nineteen. Her anus was put forward. She had a short narrow vagina, and the uterus could be easily felt on pushing up the finger into it. On looking at the bladder the two ureters could be seen, which dropped water sometimes together and

sometimes alternately, while occasionally, the water being held, there would be a squirt. He turned down a long umbilical flap, and then two pyriform flaps at each side. The flaps came nicely together. The umbilical flap fitted in by the side of those which overlapped and were brought together by hare-lip pins. For some time the case progressed favourably, but the water trickled down underneath the flaps, and there did not seem to be retention. The side flap and portion of the anterior flap sloughed. Besides having the abdominal wall open, the anterior wall of the bladder was absent. But the girl would not be operated on again unless he promised a cure. The second case was that of a young gentleman who had got ectopia vesicæ with a hypospadias. He made an excellent recovery following the operation, and he had a certain power of retention, though obliged to wear an artificial apparatus to catch the water. With regard to the use of spirits of juniper, carbohic acid, and other medicines, it was a pity Dr. Bennett had not had the opportunity of completing his experiments. He had not himself tried medicine with the young woman, but in the case of the boy there was a smell of carbohic acid from the urine. The right ureter used to drop more than the left. He asked was there in Dr. Bennett's case any dilatation of the ureters which appeared to cause the retention of the urine before the operation, and to what did Dr. Bennett attribute the death of the child? He also invited opinion as to whether, in external eversion, it would not be more desirable to turn the ureters into the rectum and allow the water to escape in that way, as the most comfortable for the patient, and least likely method of rendering the flaps liable to slough.

DR. MILES thought the most interesting point was the dilatation of the ureters from the pressure of the flaps, and suggested that it might be overcome by the contraction of the fibres of the ureter.

MR. STOKES said he had himself a case of ectopia in a female child, aged two and a-half years, under his care some years ago in the Richmond Hospital. The child's mother was irritable and discontented, and did all she could to frustrate his efforts. He was not disposed to adopt Wood's operation, there being such a redundancy of tissues on each side of the opening, and under the circumstances he considered two square-shaped flaps brought together over the mesial line would be sufficient. But following this procedure, what he was all along apprehensive of occurred—namely, the sloughing of one of the flaps, while the other remained in its position and diminished to a certain extent the opening, thus leaving the case in a better condition for future operation. The patient went home, to return at the end of three or four months, when he performed another plastic operation, taking the flaps somewhat pyriform in shape from each side of the groin. The opening was still further diminished, but not sufficiently so for material improvement, and the

patient suffered from constant dribbling of water. Accordingly he recommended a third operation, intending to carry out Wood's plan of turning down the flap and covering it in by two additional side flaps; but the child's mother lost patience, and would neither remain in hospital nor leave the child. Thus the case was left with the operation incomplete. What Dr. Bennett said with regard to the dilatation of the ureter, formed an argument rather against Wood's operation and in favour of some operation similar to that which he had performed; because the double flap placed over the defect must necessarily cause a greater amount of pressure on the ureters than two single flaps from each side drawn across the defect. Another objection to Wood's operation in the case of adults was that the turning back and having a hairy surface on the lining of the membrane of the bladder might lead to troublesome consequences by the formation of phosphatic deposits, unless steps were taken first to destroy the hair.

DR. G. H. KIDD said he had a case of external eversion or ectopia in the case of a girl, aged nineteen, at the Coombe Hospital, who had never before been in the hands of a medical man, and would not then had she not combined with the state of the bladder prolapse of the uterus, the cause of which he did not know. The vagina was small, but the uterus was of the normal size. Between the external eversion of the bladder and the opening into the vagina there was a band of skin  $1\frac{1}{2}$  in., covered with the hair of puberty. The whole surface appeared congested. On consultation operation was deemed undesirable. The vagina was large enough to enable him to put in a small ring support, which kept the uterus in position to allow the congestion to go down. The patient was now in the South Dublin Union Workhouse.

DR. BENNETT, in reply, said Mr. Wheeler's proposed alternative procedure to Wood's of turning the ureters into the rectum had been tried already by Seymour; but it induced a tendency to diarrhoea and a fatal issue by the constant entrance of urine into the rectum, producing urinary concretions. He attributed the dilatation of the ureters which caused death not, as Dr. Miles supposed, to the pressure of the abdominal flaps, which might co-operate to produce it, but to the extreme amount of cicatricial contraction developed. In only one or two cases had Wood's operation produced this result. Wood's operation was applicable, perhaps, rather to the male than to the female. When the condition of affairs was present as indicated after the first operation the child's health began to fail and the ureters to dilate. With reference to urinary concretions, they had tested qualitatively for iodide of potassium, but as to the amount of urea they never could get more than half a drachm of urine on any occasion. The question of hair did not arise in the female, as she did not develop hair above the umbilicus. Wood had recorded the growth of hair in the bladder and the accumulation of calculus upon it.

# SANITARY AND METEOROLOGICAL NOTES.

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## VITAL STATISTICS

*For four Weeks ending Saturday, March 27, 1886.*

The deaths registered in each of the four weeks in the sixteen principal Town Districts of Ireland, alphabetically arranged, corresponded to the following annual rates per 1,000 :—

Towns	Weeks ending				Towns	Weeks ending			
	March 6.	March 13.	March 20.	March 27.		March 6.	March 13.	March 20.	March 27.
Armagh -	46·5	5·2	36·1	15·5	Limerick -	31·0	39·1	33·7	28·3
Belfast -	30·3	39·3	33·6	29·1	Lisburn -	58·0	29·0	9·7	29·0
Cork -	29·2	34·4	27·9	27·3	Londonderry	21·4	25·0	39·2	32·1
Drogheda	8·5	33·8	46·5	38·1	Lurgan -	10·3	15·4	25·7	20·5
Dublin -	29·8	37·8	39·6	33·1	Newry -	21·1	21·1	21·1	10·5
Dundalk -	26·2	34·9	21·8	21·8	Sligo -	28·9	14·4	4·8	24·1
Galway -	20·2	43·7	30·3	47·1	Waterford -	13·9	41·7	41·7	32·4
Kilkenny	12·7	33·8	29·6	25·4	Wexford -	17·1	34·2	29·9	21·4

In the week ending Saturday, March 6, the mortality in twenty-eight large English towns, including London (in which the rate was 26·9), was equal to an average annual death-rate of 26·6 per 1,000 persons living; in Glasgow the rate was 30·1; and in Edinburgh 22·5. The average annual death-rate represented by the deaths registered in the sixteen principal town districts of Ireland was 28·5 per 1,000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 1·7 per 1,000, the rates varying from 0·0 in nine of the districts, to 9·6 in Sligo; the 6 deaths from all causes registered in that district comprise 1 from typhus and 1 from diarrhoea. The 129 deaths from all causes registered in Belfast comprise 2 from measles, 2 from scarlatina, 1 from whooping-cough, 1 from diphtheria,

and 1 from enteric fever; and the 23 deaths in Limerick comprise 1 from typhus and 1 from whooping-cough.

In the Dublin Registration District the births registered during the week amounted to 166—81 boys and 85 girls—and the deaths to 204—106 males and 98 females.

The deaths represent an annual rate of mortality of 30·1 in every 1,000 of the estimated population; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 29·8 per 1,000.

Seventeen deaths from zymotic diseases were registered, being 12 below the average for the corresponding week of the last ten years, and 7 under the number for the week ended the 27th ultimo; they comprise 1 from typhus, 10 from whooping-cough, 3 from enteric fever, 1 from diarrhœa, &c.

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In the week ending Saturday, March 13, the mortality in twenty-eight large English towns, including London (in which the rate was 28·7), was equal to an average annual death-rate of 28·0 per 1,000 persons living; in Glasgow the rate was 33·6; and in Edinburgh 21·4. The average annual death-rate in the sixteen principal town districts of Ireland was 36·2 per 1,000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 2·5 per 1,000, the rates varying from 0·0 in Galway, Newry, Drogheda, Wexford, Dundalk, Sligo, Lisburn, and Armagh, to 5·1 in Lurgan; the 3 deaths from all causes registered in the last-named district comprise 1 from typhus. The 167 deaths from all causes registered in Belfast comprise 4 from measles, 2 from whooping-cough, 1 from enteric fever, and 1 from diarrhœa; among the 53 deaths in Cork are 1 from measles, 2 from scarlatina, 1 from typhus, 1 from enteric fever, and 2 from diarrhœa; the 14 deaths in Londonderry comprise 2 from whooping-cough; and the 18 deaths in Waterford comprise 1 from enteric fever and 1 from diarrhœa.

In the Dublin Registration District the births registered during the week amounted to 225—107 boys and 118 girls—and the deaths to 259—120 males and 139 females.

The deaths represent an annual rate of mortality of 38·2 in every 1,000 of the estimated population; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 37·8 per 1,000.

Twenty-four deaths from zymotic diseases were registered, being 7 over the number for the preceding week, but 7 under the average for the 10th week of the last ten years; they comprise 1 from scarlet fever (scarlatina), 3 from typhus, 11 from whooping-cough, 3 from enteric fever, 1 from diarrhœa, 2 from erysipelas, &c.

In the week ending Saturday, March 20, the mortality in twenty-eight large English towns, including London (in which the rate was 30·3), was equal to an average annual death-rate of 29·3 per 1,000 persons living; in Glasgow the rate was 33·9; and in Edinburgh 23·9. The average annual death-rate in the sixteen principal town districts of Ireland was 34·9 per 1,000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 1·7 per 1,000, the rates varying from 0·0 in ten of the districts to 5·3 in Londonderry; the 22 deaths from all causes registered in that district comprise 2 from whooping-cough and 1 from enteric fever. The 143 deaths from all causes registered in Belfast comprise 2 from measles, 1 from scarlatina, 2 from whooping-cough, 1 from simple continued fever, and 2 from diarrhœa; and the 43 deaths in Cork comprise 1 from each of the following diseases, viz.:—Measles, scarlatina, and diarrhœa.

In the Dublin Registration District the births registered during the week amounted to 181—90 boys and 91 girls—and the deaths to 274—135 males and 139 females.

The deaths represent an annual rate of mortality of 40·5 in every 1,000 of the estimated population; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 39·6 per 1,000.

Twenty-one deaths from zymotic diseases were registered, being 8 below the average for the corresponding week of the last ten years, and 3 under the number for the week ended 13th instant; they comprise 6 from whooping-cough, 1 from cerebro-spinal fever, 6 from enteric fever, 1 from diarrhœa, 2 from erysipelas, &c.

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In the week ending Saturday, March 27, the mortality in twenty-eight large English towns, including London (in which the rate was 26·9), was equal to an average annual death-rate of 26·4 per 1,000 persons living. In Glasgow the rate was 30·5; and in Edinburgh 23·7. The average annual death-rate in the sixteen principal town districts of Ireland was 30·3 per 1,000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 2·1 per 1,000, the rates varying from 0·0 in nine of the districts to 4·8 in Lisburn; the 6 deaths from all causes registered in that district comprise 1 from enteric fever. The 124 deaths from all causes registered in Belfast comprise 2 from scarlatina, 3 from whooping-cough, and 6 from diarrhœa.

In the Dublin Registration District the births registered during the week amounted to 203—106 boys and 97 girls—and the deaths to 234—114 males and 120 females.

The deaths represent an annual rate of mortality of 34·6 in every 1,000



of the estimated population; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 33·1 per 1,000.

Twenty-one deaths from zymotic diseases were registered, being equal to the number for the preceding week, but 14 under the average for the 12th week of the last ten years; they comprise 1 from scarlet fever (scarlatina), 12 from whooping-cough, 1 from enteric fever, 4 from diarrhœa, 1 from erysipelas, &c.

#### METEOROLOGY.

*Abstract of Observations made in the City of Dublin, Lat. 53° 20' N., Long. 6° 15' W., for the Month of March, 1886.*

Mean Height of Barometer,	-	-	-	29·880 inches.
Maximal Height of Barometer (on 11th, at 9 a.m.)	-	-	-	30·463 „
Minimal Height of Barometer (on 31st, at 7 a.m.)	-	-	-	29·200 „
Mean Dry-bulb Temperature,	-	-	-	41·2°.
Mean Wet-bulb Temperature,	-	-	-	38·7°.
Mean Dew-point Temperature,	-	-	-	35·2°.
Mean Elastic Force (Tension) of Aqueous Vapour,	-	-	-	·216 inch.
Mean Humidity,	-	-	-	79·3 per cent.
Highest Temperature in Shade (on 21st),	-	-	-	60·4°.
Lowest Temperature in Shade (on 4th),	-	-	-	23·0°.
Lowest Temperature on Grass (Radiation) (on 4th),	-	-	-	17·3°.
Mean Amount of Cloud,	-	-	-	61·7 per cent.
Rainfall (on 19 days),	-	-	-	2·065 inches.
Greatest Daily Rainfall (on 26th),	-	-	-	·462 inch.
General Directions of Wind,	-	-	-	S.E., E., S.W.

#### Remarks.

The bitterly cold weather, which had prevailed during the greater part of January and February, held until the 18th of March, when a magical change occurred throughout the British Islands. From the day named until the 29th the mean temperature was as much above, as it had previously been below, the average, and hence it happens that March, 1886, will not stand out prominently as a cold month in the record of many years. In Dublin the mean temperature of the month was lower than that recorded in 1886 (41·4°) on five occasions in the twenty years, 1865-84—viz., in 1865 (41·2°), 1867 (39·0°), 1869 (41·2°), 1876 (41·1°), 1883 (39·0°). At the same time, no such continuous and severe cold in March was noted in any of those twenty years as that which held until the 18th of the past month. The first week was, in fact, the coldest period of the winter of 1885-86—the mean temperature being only slightly above freezing point,

The mean height of the barometer was 29·880 inches, or 0·043 inch below the average value for March—namely, 29·923 inches. The mercury rose to 30·463 inches at 9 a.m. of the 11th, and fell to 29·200 inches at 7 a.m. of the 31st. The observed range of atmospherical pressure was, therefore, 1·263 inches—slightly more than an inch and a quarter. The mean temperature deduced from daily readings of the dry bulb thermometer at 9 a.m. and 9 p.m. was 41·2°, or 2·1° above the value for February, 1886; that calculated by Kaemtz's formula—viz.,  $\text{min.} + (\text{max.} - \text{min.} \times \cdot 41) = \text{Mean Temp.}$ —from the means of the daily maxima and minima was 40·5°, or 2·2° below the average mean temperature for March, calculated in the same way, in the twenty years, 1865–84, inclusive (42·7°). The arithmetical mean of the maximal and minimal readings was 41·4°, compared with a twenty years' average of 43·5°. On the 21st the thermometer in the screen rose to 60·4°—wind S.W.; on the 4th the temperature fell to 23·0°—wind calm. The minimum on the grass was 17·3° on the same date. The diurnal range of temperature was small on many occasions during the cold period. The rainfall was 2·065 inches, distributed over 19 days. The average rainfall for March in the twenty years, 1865–84, inclusive, was 2·081 inches, and the average number of rainy days was 16·5. The rainfall, therefore, was slightly below, while the rainy days were decidedly above the average.

Sleet or snow fell on the 1st, 2nd, 3rd, 14th, 16th, 17th, 18th, 29th, and 31st. Hail was noted on the 2nd, 3rd, 14th, 15th, 16th, 17th, 29th, and 31st. The air was more or less foggy on the 4th, 13th, 18th, and 22nd. Lunar halos were observed on the night of the 19th, and solar halos appeared on the 7th and 10th. High winds prevailed on as many as fourteen days. A bright aurora borealis was seen on the evening of the 30th, and a fainter display occurred on the 31st.

The month opened with a snowstorm of exceptional severity, connected with a deep depression, which appeared over St. George's Channel on the 1st, and moved towards E.N.E., until its centre reached the mouth of the Humber on the 2nd. Snow lay five inches deep in the streets of Dublin, but in the N. and N.E. of England vast drifts completely blocked the traffic by rail and road. During the first week the weather remained in a most winter-like condition—the prevailing winds being easterly to northerly, the temperature low, and the air dry. Falls of snow and sleet were of frequent occurrence. On the 4th the frost in Dublin was more intense than it had been at any time during the winter, and the mean temperature of the week was only 33·1°.

In the week ending Saturday, the 13th, there was a continuance of cold weather, particularly over Great Britain and the North Sea, where keen, dry easterly winds prevailed. On the 7th an intense frost occurred in the N.W. of England in the rear of a depression, which had passed

south-eastwards across France during the previous day. At Tean (Stoke-upon-Trent) the sheltered thermometer fell to  $7^{\circ}$ , at Worksop to  $8^{\circ}$ , and at Newton Reigny to  $10^{\circ}$ . At Worksop an instrument exposed on the snow descended to  $-5^{\circ}$ . In Ireland, about this time, strong S.E. winds and gales prevailed. On Tuesday, the 9th, the wind was very searching and the atmosphere was unusually dry in Dublin—the relative humidity at 9 p.m. being only 50 per cent.

In the course of the week ending Saturday, the 20th, one of the most sudden and complete changes of weather experienced for many years took place throughout the United Kingdom. The period commenced with a continuation of the frosty, gloomy conditions which had held for so long. In and near Dublin hail, sleet and snow fell daily up to and including Wednesday, the 17th, and in Great Britain the nights were severely cold. During the forenoon of Wednesday, however, the barometer fell in the west, and later in the day a large depression advanced rapidly towards Ireland. Its centre moved north-eastwards outside our western coasts, so that the wind drew into S. and S.W. all over the Kingdom, and warm showery weather spread quickly across the country. Thus, whereas at Oxford the thermometer had been as low as  $21^{\circ}$  early on the 18th, it had risen to  $55^{\circ}$ —a range of  $34^{\circ}$ —by the afternoon of the 19th, when also a maximum of  $62^{\circ}$  was attained in London—the highest reading there recorded since September 29, 1885.

Throughout the fourth week (the 21st to the 27th inclusive) the weather was changeable, mild and damp, with southerly (S.E. to S.W.) winds, which, though moderate generally, freshened at times, especially in the West of Ireland, where they reached the force of a gale. During this period the barometer was low over the Atlantic, high on the Continent, the distribution of pressure being mainly cyclonic in the west. Temperature was considerably above the average—the mean for the week in Dublin was  $52.5^{\circ}$ , and maximal readings, varying from  $60^{\circ}$  to  $65^{\circ}$  were recorded in many places. A heavy fall of rain occurred in Ireland on the 26th.

During the last three days several depressions passed across this country. The weather was very broken, with unsteady temperature, severe gales from S.W. to N.W., showers of cold rain, sleet and hail, in places thunder and lightning, and brilliant aurora at night. The auroral display of the 30th was seen from Dublin as a broad band of moonlike light projected across the northern sky from W. to E. On the evening of the 31st white streamers rose from the north from time to time.

## PERISCOPE.

### THE ANTIPHLOGISTIC ACTION OF MENTHOL.

DR. S. RUSSEL of Albany, New York, writes as follows to the *Medical Record*, Nov. 21st, 1885, to call attention to the use of menthol as an antiphlogistic and anodyne remedy in superficial inflammations, in which he says its effects are truly remarkable:—"My discovery of its value in such conditions was quite accidental; a patient who was suffering from acute aural perichondritis, failing to obtain relief from the usual anodyne applications, bethought himself of menthol, which he applied by means of the ordinary pencil; the relief from pain was immediate, and from that time, to my surprise, the inflammation quickly subsided. I have since made frequent use of it, and I believe inflammations have aborted which without it would have ended in abscess, and in addition the pain, itching, heat, and swelling have been considerably reduced. I have used the ethereal solution (alcohol is also a good solvent) in the strength from 10 to 50 per cent., applying it by means of a camel's hair pencil. The application may be made two or three times a day, or more or less frequently, according to the indications. A solution in one of the fixed oils may be used on mucous membranes. I have observed the good effects of menthol, more especially in boils, carbuncles, excessive inflammation following vaccination, &c. The employment of this substance in superficial neuralgias for its anodyne effect alone is often very satisfactory, but its use in this way has been noticed and written upon by others."—*Therapeutic Gazette*, January, 1886, and *Ed. Med. Jour.*

### HYDROCHLORATE OF COCAIN IN THE VOMITING OF PREGNANCY.

WEISS, of Prague, has used this remedy successfully in a case of vomiting of pregnancy which had resisted all previous attempts at relief. The patient was weak and anæmic, of a nervous disposition, and had suffered in three previous pregnancies from persistent vomiting. In the present pregnancy her condition was serious. Weiss prescribed:—*R.* Hydrochlorate of cocain, gr. 2; alcohol, enough to dissolve; water,  $\mathfrak{z}\text{v}$ . *S.*—One teaspoonful every half-hour. After the sixth dose three tablespoonfuls of milk were well borne; after the eighth, a cup of broth with egg, without vomiting. After the sixteenth dose the patient ate with relish chicken broth, slices of white chicken meat, and drank a glass of wine, without vomiting. The drug was then withdrawn for a time, owing to an increased frequency of pulse and respiration; but hourly doses were subsequently given, with the result of entirely checking the vomiting and enabling the patient to regain her former strength.

The *Edinburgh Medical Journal* for March, from which we take the foregoing extract, also gives, from the *Archiv. de Tocol.*, Sept., 1885, a case of excessive vomiting of pregnancy which was instantly relieved, after the most varied means hitherto in vogue had failed to arrest, by irrigation (?) of ether upon the epigastrium. [*Vide* page 356 *suprà*.]

#### RUPTURES OF THE HEART.

AT a recent meeting of the Société Médicale des Hôpitaux of Paris, Dr. A. Robin made some remarks on ruptures of the heart (*Revue de Thérapeutique Médico-Chirurgicale*, Jan. 15). As is well known, ruptures of the heart occur only in more advanced age. They are brought about by sclerotic myocarditis of vascular origin, and cause sudden death, it being in most cases impossible to assign specific symptoms to this disease. Dr. Robin met successively with three cases of rupture of the left ventricle, which were highly interesting for several reasons. In every one of these three cases, the patients dropped down dead whilst apparently in perfect health, there not having been the slightest apprehension of disease of the heart. The *post mortem* examination proved in each case the presence of sclerotic myocarditis, a disease which had been latent during its development. It was further found that the ruptures did not take place from within outwards, but from without inwards, and that they formed slowly and progressively, and not suddenly. It seems that ruptures of the heart may enlarge by degrees, from the cases under Dr. Robin's observation, and from 60 per cent. of those published, in which death was preceded by some characteristic prognostics—viz., præcordial pain radiating to the left shoulder and arm, dyspnoea, anxiety, &c. These symptoms present themselves in the form of acute attacks, resembling those of angina pectoris. They have been erroneously explained by a stoppage of blood in the pericardium; but this cannot be the case, as there is generally a considerable interval between the commencement of the attacks of angina and death. Moreover, the auscultation during this interval remains negative, whilst the symptoms due to extravasation of blood in the pericardium are entirely different, as Robin had the opportunity of ascertaining in a case of rupture of the coronary artery, which took place whilst he was examining the patient, who was suddenly seized by a violent attack of orthopnoea and tumultuous palpitations. On auscultation, peculiar sounds, like those of surging waves, were perceived. Death ensued after a few hours. There is consequently no mistake possible between the symptoms of these two different injuries. In summing up, Dr. Robin wishes to draw attention to the following important points: 1. The latent state of the myocarditis; 2. the possibility of rupture from without inwards; 3. the symptoms specific to rupture of the heart; and 4, the difference of the symptoms of rupture of the heart and extravasation of blood in the pericardium.—*London Med. Record*, March 15.

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OF

## MEDICAL SCIENCE.

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# THE DUBLIN JOURNAL

OF

## MEDICAL SCIENCE.

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JUNE 1, 1886.

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### PART I.

### ORIGINAL COMMUNICATIONS.

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ART. XXIV.—*The Cardiac Murmurs of the Mitral Area.*\* By  
C. J. NIXON, M.B., LL.D. Univ. Dubl.; M.D. and F.R.U.I.;  
F.K.Q.C.P.; Senior Physician to the Mater Misericordiæ  
Hospital, &c.

IN discussing the varieties of murmur which are developed in connection with the mitral valve, and audible over the mitral area, I purpose to deal chiefly with the special points of interest in connection with the murmurs themselves, not with concomitant symptoms of the lesions which they indicate. The murmurs to which I shall direct special attention are:—

- I. PRESYSTOLIC MURMUR.
- II. POSTDIASTOLIC MURMUR.
- III. ORGANIC SYSTOLIC MURMUR.
- IV. FUNCTIONAL SYSTOLIC, OR POSTSYSTOLIC, MURMUR.
- V. MITRAL BRUIT DE SCIE.

#### I. PRESYSTOLIC MURMUR.

The first two murmurs, it is needless to say, are the common physical signs of mitral stenosis, and it is necessary to study certain peculiarities of each. Taking, first, presystolic murmur, the points to which I would specially direct attention are—its site of maximal intensity, its acoustic characters, the conditions of the mitral valve with which it is connected, and the signs which may simulate it. Since Fauvel and Gendrin first connected this

\* Read before the Medical Section of the Academy of Medicine in Ireland, Friday, April 30, 1886.

murmur with a constricted mitral orifice, no physical sign of cardiac disease has been more thoroughly investigated, and none rests on a more secure basis as a sign pathognomonic of a certain physical condition. There may be some difference of opinion amongst physicians as to the precise acoustic character of the murmur—a difference which to some extent depends upon the fertility of the imagination in determining resemblances to the nature and quality of sounds generally. Thus, one writer remarks, that since the time of Laennec we have had cardiac sounds described as filing, grating, rasping, croaking, crowing, whining, caterwauling and blubbering. With such an *embarras de richesses* it is not surprising that some observers regard presystolic murmur as blubbering in quality—others, as rasping, as resembling the symbols, Rrrb, voot, or rup. I think, however, the term which best conveys the idea of its acoustic character is that it is vibratile. These differences of opinion, trivial and unimportant as they are, in no way influence the general acceptance of the murmur as a positive sign of mitral stenosis. Its site of maximal intensity is usually at a point slightly internal to the apex beat, though in many cases it is heard with great distinctness well to the left of the apex, on a line with the nipple. In very exceptional cases the murmur is carried downwards to the tricuspid area, as in two cases recorded by Dr. Sansom, where it was conveyed by convection from a calcareous auriculo-ventricular ring along the inter-ventricular septum to the base of the ensiform cartilage.

It may, no doubt, appear heterodox to deny that this murmur is invariably associated with stenotic disease of the mitral orifice, and yet I confess that, in some cases of extreme palpitation of the heart of a temporary character, I have heard an intersional apex murmur which, from its rhythm, was apparently presystolic. This murmur was, however, developed under such conditions of cardiac excitement that I scarcely feel warranted in urging the view of the occurrence of a functional presystolic murmur. I would merely ask—Can a murmur be developed in such cases from a slight amount of regurgitation taking place just at the moment of commencement of ventricular systole, before the valves are forcibly brought into perfect apposition and, consequently, before their condition of tension is reached?

A mode of production of this murmur, independent of any organic disease of the mitral valve, has been urged by the late Dr. Austin Flint, which deserves some notice. I cannot, indeed,

refer to this view without an expression of regret that the distinguished physician who enunciated it has, within the last few weeks, suddenly passed away, leaving a record in work done of which his countrymen may be proud. Dr. Flint holds that in some cases of aortic patency the mitral curtains are floated together by the regurgitating stream of blood, so that the mitral direct current passing between the curtains throws them into vibration, and gives rise to the characteristic blubbery murmur. In connection with this view I may observe that it has occurred to me more than once in examining the heart to find, over the mitral area, what was apparently a presystolic murmur of mitral stenosis; on listening over the aortic area a well-marked *bruit de scie* was audible. In those cases I have been able to satisfy myself that the diastolic basic murmur conducted to the mitral area gave a deceptive idea of the existence of a mitral direct murmur. In reference to this point I may quote an observation of Walsh:—“A murmur may fail to present the precisely same synchronism at different spots of the chest-wall, even within the limits of the deep-seated cardiac region. Thus, the rhythm of a reflux aortic murmur, precisely diastolic at the base, may cease to be so at the ensiform cartilage, as also, if audible there, at the left apex. At these points it synchronises more with the long pause, and approaches the first sound, or inclines to trench on systolic time.”\* I shall merely add that if a presystolic murmur could be produced in the way suggested by Dr. Flint, it would be difficult to explain its absence in all cases of aortic regurgitation where there was much dilatation of the left ventricle.

In noting the different conditions of the cardiac sounds with which presystolic murmur may co-exist, I believe the following order will represent the frequency of the changes produced:—

- (a) Presystolic murmur, with a sharp, clicking, first sound.
- (b) Presystolic murmur, with a systolic murmur of mitral reflux.
- (c) Presystolic murmur, with partial or complete extinction of aortic second sound at apex.
- (d) Presystolic murmur, preceded by postdiastolic murmur.
- (e) Presystolic murmur, with rough functional pulmonary murmur.

In all these conditions there is generally a marked accentuation of the pulmonary second sound, and in the majority of cases this sound is doubled.

\* Diseases of the Heart, Dr. Walsh. 4th edition. Page 79.

There can be no doubt but that mitral stenosis, of a marked degree, may exist without the development of any murmur, with the presence merely of a mitral systolic murmur, or simply with a clicking first sound. Usually the absence of the direct murmur is due to weakness of the heart generally, and of the left auricle specially. Its absence has been noted when tricuspid regurgitation takes place, so that the tension of the left auricle is thus relieved. Frequently the direct murmur can be reproduced by getting the patient to make some exertion, as sitting up suddenly in bed, swinging the arms about, &c. In like manner, a presystolic thrill can be developed or intensified by getting the patient to sit up and lean to the left side; the thrill is then readily appreciable to the hand placed over the mitral area. In some cases the absence of murmur may be accounted for by taking into consideration the change in the position of the left ventricle owing to extreme hypertrophy and dilatation of the right side of the heart. The left side is completely dwarfed in size, does not reach the surface of the chest, hence the vibrations of sound developed at the diseased valve are not audible. The disappearance of presystolic murmur is, however, in most cases a temporary phenomenon; after an absence of several days it often re-appears with marked intensity without any assignable cause. Figs. 1 and 2 afford graphic illustrations of presystolic murmur followed by clicking first sound, or by a systolic murmur.\*

## II. POSTDIASTOLIC MURMUR.

Closely connected with presystolic murmur comes the study of postdiastolic murmur. I use the term "postdiastolic" advisedly, as it is the accurate chronometric description of the murmur. If the principle of intersonal murmurs be admitted, there can be no objection to the use of the term "postdiastolic." Both presystolic and postdiastolic murmurs are, no doubt, diastolic murmurs, if we regard them solely in relation to the condition of the left ventricle during the period of their development. Theoretically, there is some confusion in describing mitral murmur as diastolic, because it is the habit to speak of the sounds of the heart as systolic and diastolic, and to connect murmurs strictly with morbid conditions affecting the parts of the heart at which those sounds are developed. For this reason I ventured to suggest, in a communication on

\* For the preparation of the diagrams I am indebted to Mr. Ambrose E. J. Birmingham, Demonstrator of Anatomy in the Medical School of the Catholic University.

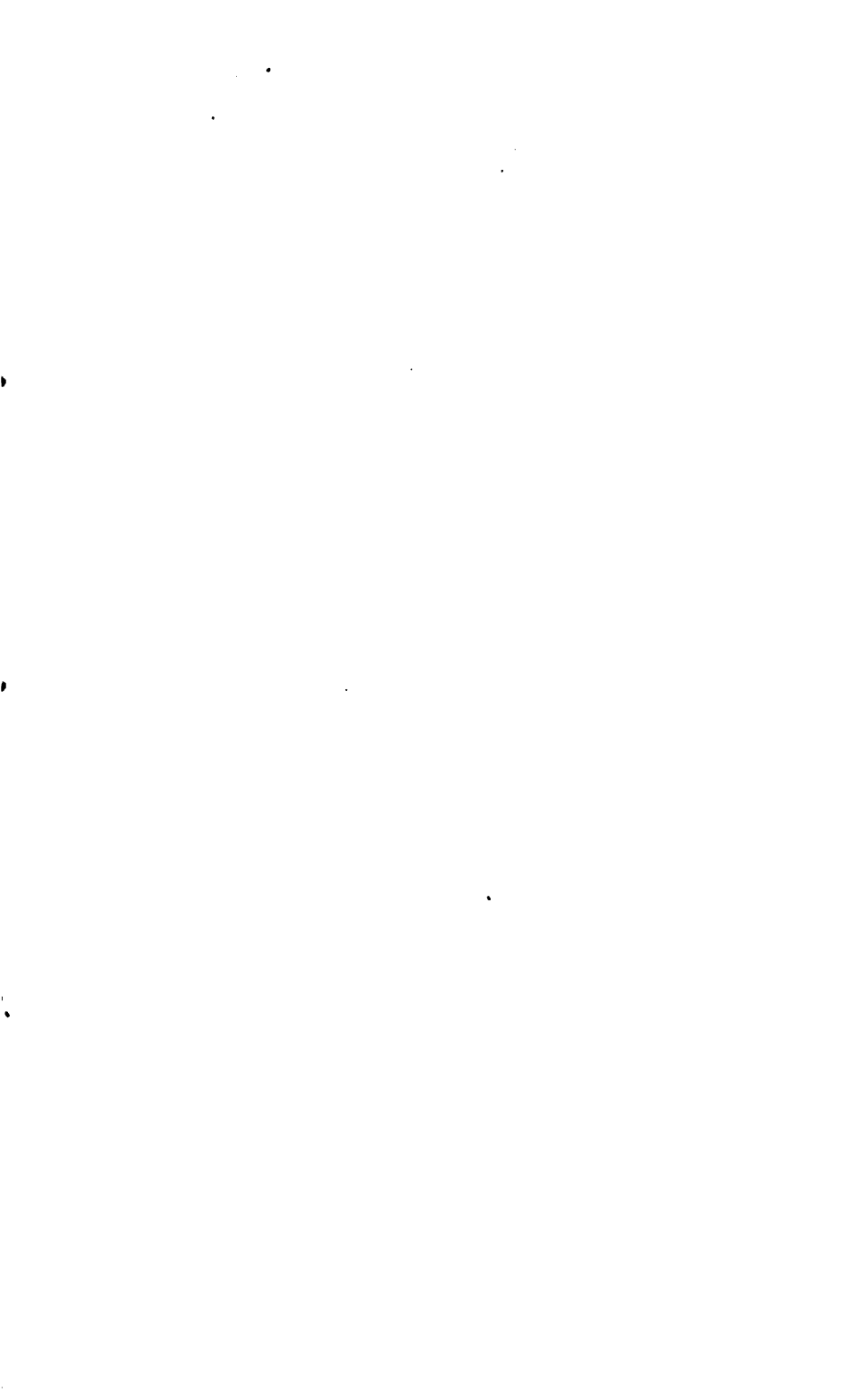


Fig. 1.

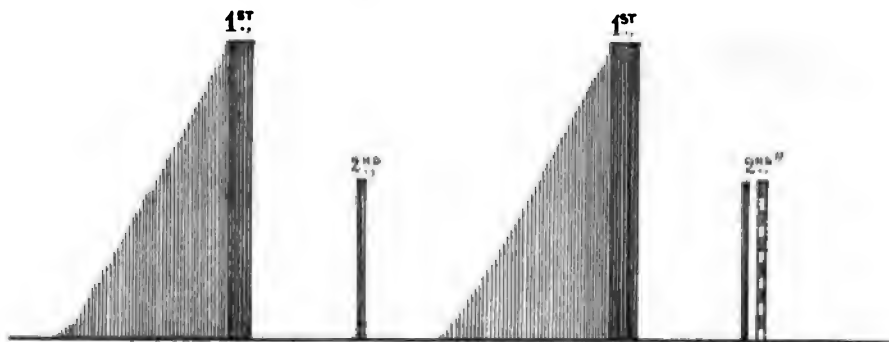


Fig. 3.

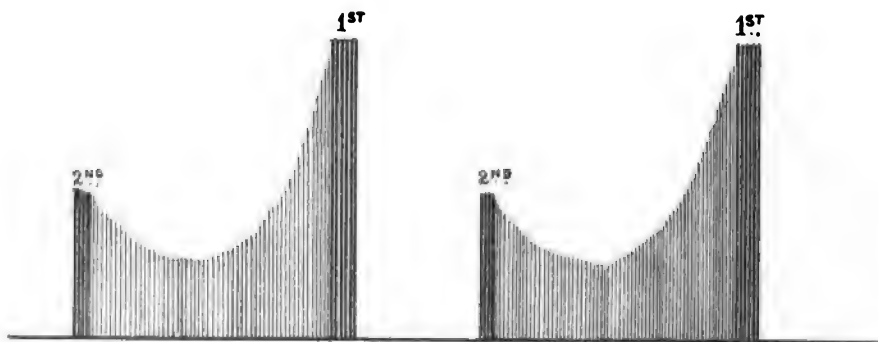


Fig. 5.

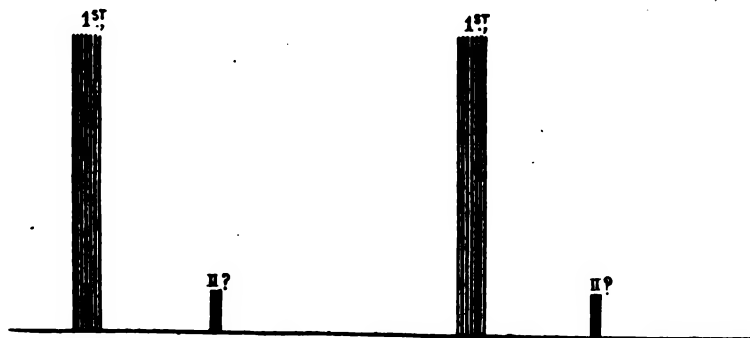


Fig. 2.

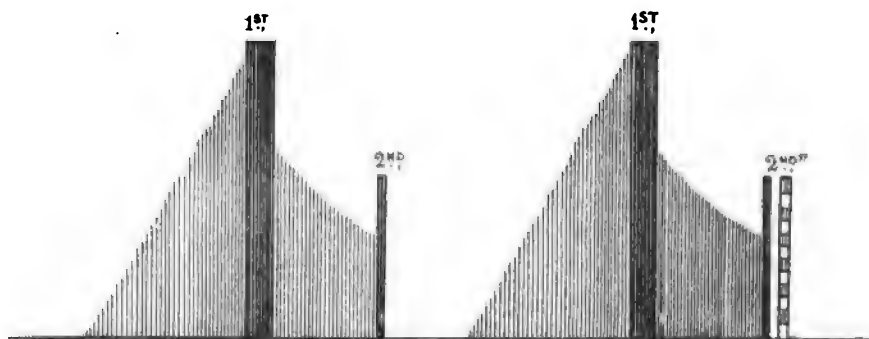


Fig. 4.

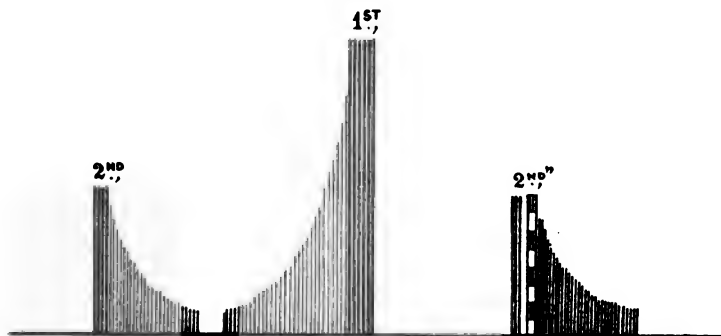
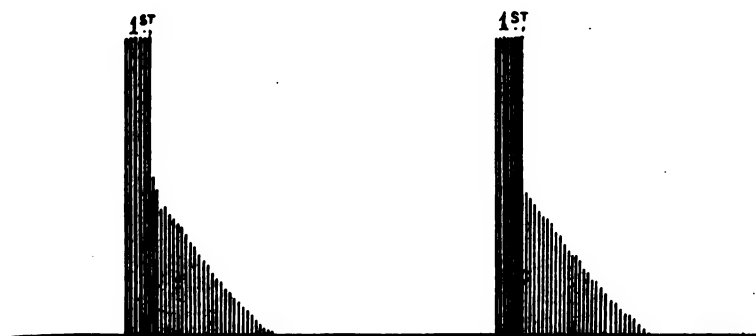


Fig. 6.







mitral stenosis which I brought before the Medical Society of the College of Physicians, that presystolic murmur might be considered as occurring in two forms—a short presystolic murmur, corresponding to the auricular systolic murmur of Gairdner, and a long presystolic murmur, where the morbid sound occupied the entire of the long pause; but I fully recognise that this nomenclature does not satisfy the conditions met with in the disease. Overlooking a merely theoretical objection, we find that the murmur under consideration is so closely related in time to the second sound, that the correctness of associating it with that sound must be admitted. As the murmur does not influence directly the mechanism of the production of the second sound, and as it distinctly follows it, instead of being exactly timed with it, or replacing it, the term “postdiastolic” is preferable, in my opinion, to “diastolic” murmur. If the existence of the murmur be determinable at a certain precise period of the cardiac rhythm by accurate clinical observation, it is irrational to ignore its presence on the ground of over-refinement, or hyper-division of the heart sounds. Postdiastolic murmur, in contrast to presystolic murmur, is smooth and suction-like in character. Its significance is, that it indicates an extreme amount of mitral narrowing, and its recognition consequently becomes a matter of importance. In extreme mitral narrowing, the tension in the left auricle, during the momentary arrest of the flow of blood from it into the ventricle during the contraction of the latter, must be considerably increased. The left ventricle actively dilates under a negative pressure, according to Goltz and Gaule, of 23·5 millimetres of mercury (the “vacuite postsystolique” of Marey); so that fluid veins are formed, and produce a sound of sufficient intensity to render a murmur audible.\* Usually this murmur runs through the entire of the long pause, becoming faintly pronounced in the middle of it, and finally terminates in the rough, presystolic murmur. The graphic representation of the murmur may be given as shown in Fig. 3.

Frequently this long murmur is split into two, there being a distinct break in the continuity of the sound in the middle of the long pause, when the murmur may be represented as in Fig. 4. Or the presystolic murmur may be entirely absent, when the signs of stenosis are represented by a clicking first sound, with

\* Taking into consideration the mechanism of the production of postdiastolic murmur, it might be appropriately designated a ventricular diastolic murmur, whilst the presystolic murmur would be correctly termed an auricular systolic murmur.

partial or complete extinction of the aortic second sound, or with a clicking first sound succeeded by a systolic murmur, as in Figs. 5 and 6. Any of the conditions just mentioned may be associated with a systolic murmur of mitral inadequacy.

Dr. Broadbent, in a recent valuable and interesting communication on mitral stenosis, regards the development of the signs of this affection as divisible into three stages following each other in a fixed order:—

1st stage—Rough, vibratile, presystolic murmur, accentuated pulmonary second sound, aortic second sound audible at or beyond apex.

2nd stage—Presystolic murmur, with short, sharp first sound, and disappearance of second sound at apex.

3rd stage—Disappearance of the presystolic murmur, a loud, sharp mitral first sound with or without a systolic tricuspid murmur.

I am not quite prepared to admit the chronological development of these signs, as given by Dr. Broadbent, though I freely concede the existence of the various signs he mentions at different periods of the disease. By a curious coincidence, when writing these lines on Sunday last, I was asked by Dr. Delahoyde to see in consultation with him a clergyman who presented what I regard as the typical signs of mitral stenosis of long duration. Twenty years ago the patient had been told by Sir Dominic Corrigan that he had heart disease, and he was specially warned to avoid violent exercise, &c.

The signs noted at the time of my visit were:—Apex beat displaced about one inch to left side, and slightly upwards; short presystolic thrill terminated by a sharp knock over site of impulse beat. At apex, on auscultation, a short, rough presystolic murmur audible, followed by clicking first sound, and a faintly pronounced second sound. At a point about an inch to the left of the impulse beat, and somewhat above it, the murmur was heard with great distinctness followed by the sharp first sound, but in this situation the second sound was no longer audible. At the base of the heart the second sound was doubled, the pulmonic element being distinctly accentuated. The usual signs of hypertrophied and dilated right ventricle existed.

Two other points in connection with the signs of mitral stenosis remain for a brief consideration—the alteration of the first sound which is usually noted, and the partial or complete suppression of the aortic second sound at the apex.

With reference to the first point, I may mention that I do not think there are good grounds for accepting Dr. Silver's view, that

the presence of a sharply accentuated first sound preceded by a presystolic murmur is an indication of a funnel-shaped as distinguished from a diaphragmatic mitral valve. I cannot conceive the segments of the mitral valve producing by apposition of their surfaces, if such a condition could take place, a sound like that under consideration. One of the best marked cases of a diaphragmatic mitral valve, with the usual button-hole slit, which ever came under my observation, was evidenced during life by a presystolic murmur and clicking first sound—the specimen I had the honour of exhibiting before the Medical Society of the College of Physicians.

It appears to me that the alteration in the first sound in mitral stenosis is due entirely to the tension and vibrations of the smooth, greatly thickened but flexible mitral curtains. We have here to take into consideration the difference between the initial and final tension of the mitral valve, upon which the intensity of the valvular sound depends. In cases of aortic patency Traube has called attention to the suppression of the first sound at the apex of the heart. Owing to the regurgitation of blood into the ventricle, there is such an approximation of the initial to the final tension of the valve segments, that sound may not be sufficiently produced so as to be audible. In extreme mitral narrowing, on the other hand, the amount of blood in the ventricle is considerably diminished, there is a very slight amount of initial or presystolic tension of the mitral valve produced during the auricular contraction, so that if the ventricle contract with vigour, the difference between this and the final tension may be so great as to produce the intensified sound. I am not sure that this explanation is satisfactory in all respects, but I believe it is, at least, a condition, if not a cause of the phenomenon in question.

That the second aortic sound is greatly lessened in intensity, especially at the apex, in mitral stenosis, no one will deny. The difficulty in determining its existence at the mitral area, taken in connection with the altered character of the first sound, has been a fruitful source of error with regard to the timing of presystolic murmur. The diminished intensity of the second aortic sound is obviously due to decrease of tension in the left ventricle and aorta. I must, however, add, that in my experience, contrary to that of Dr. Broadbent, the complete disappearance of the second sound at the apex is rare in mitral stenosis. I believe there are but few cases where some evidence, however faint, of the existence of a second sound at the apex cannot be appreciated. In

such cases the sound completely vanishes at a point removed, but slightly, to the left of the impulse beat.

The partial or complete extinction of the aortic second sound is a sign of great importance in the diagnosis of cases of mitral constriction unattended with murmur, or existing with a murmur of mitral reflux.

The conditions which are most apt to simulate presystolic murmur are pericarditis, with a single friction sound limited to the area of the cardiac apex, and occurring before the impulse; and reduplication of the first sound. In cases of renal disease with cardiac hypertrophy the sign first described by Potain under the name of *bruit de galop* is frequently met with. Three sounds are heard at the apex—the two first of which represent a doubled first sound, one element of which may be taken for a presystolic murmur. In the absence of a doubled second sound, Johnson holds that the presystolic sound represents the contraction of the hypertrophied left auricle. The acoustic character of the doubled sound, the accentuation of the aortic second sound, and the signs of general hypertrophy of the heart, associated with albuminuria, should be sufficient to indicate the nature of the lesion.

### III. SYSTOLIC MITRAL REGURGITANT MURMUR.

Since Elliotson first described the murmur of mitral regurgitation in 1830, a very important change of opinion regarding its significance has taken place. It was for a long time regarded as a positive sign of organic disease of the mitral valve, and the patient in whom the sign was recognised, although he might eat, drink, and sleep well, was always regarded as a sort of latent medical volcano from which an eruption of symptoms dangerous to life might at any time take place. Of late years, however, this view has been shown to be erroneous, and the occurrence of a mitral systolic murmur from functional causes is freely conceded.

Taking, first, for consideration the murmur of organic mitral reflux, we find that it is heard at its maximal point of intensity over the mitral area, and for a short distance horizontally to the left of this point. When well pronounced it is usually transmitted towards the left anterior axillary line, and it is frequently audible in the interscapular region and at the inferior angle of the left scapula. It is usually followed, when compensating hypertrophy of the right ventricle has taken place, by an accentuated pulmonary second sound; occasionally by the *bruit de ruppel*. The existence

of thrill over the cardiac apex accompanying the murmur is frequently detectable, though the phenomenon of apex thrill is much more commonly met with in mitral stenosis. In a certain proportion of cases there can be no doubt but that the murmur of organic mitral regurgitation, instead of being developed at its point of maximal intensity over the mitral area is heard best in the direction of the pulmonary area. This was specially noted in 1844 by Skoda,\* who thought the murmur was due to some changes in the intima of the pulmonary artery. Meyer subsequently described the peculiarities of the murmur, but it attracted the special attention of Naunyn in 1868, and it is now generally known by the appellation of Naunyn's murmur. The point of maximal intensity of this murmur is in the second left intercostal space, about one and a-half inches from the edge of the sternum; consequently it is slightly removed from the pulmonary area, and corresponds to the situation of the left auricular appendix. It seems to me that too much significance is attached to the murmur, as it is, in comparison with the cases where an organic systolic mitral murmur is heard best at the apex, very rare; and I believe the explanation of its transmission towards the direction of the auricular appendix is that it is due merely to such alterations in the mitral valve as determine, by convection, the development of the murmur in the situation indicated, just as in the cases reported by Dr. Sansom, already alluded to, where, from calcareous deposition in the left auriculo-ventricular orifice, a presystolic mitral murmur was conveyed to the tricuspid instead of to the mitral area. Theoretically, following the law that murmurs are developed in the direction of the circulating blood, the murmur of mitral regurgitation should be best heard, not at the apex of the heart, but higher up over the body of the left ventricle, or in the situation of the left auricle. The attachments of the chordæ tendineæ to the diseased valve serves, however, to conduct downwards the vibrations of sound to the muscoli papillares, these to the muscular walls of the left ventricle, and so the murmur is usually most audible in the mitral area, except in the cases referred to by Naunyn. Dr. Balfour holds the peculiar view that Naunyn's murmur is frequently met with in the dilatation of the heart consecutive to spanæmia, such as is developed in chlorosis, progressive pernicious anæmia, or such like conditions. Dr. Balfour believes that the murmur of mitral reflux is carried towards the pulmonary area

\* Abhandlung über Percussion und Auscultation. Wien 1844.

through a dilated left auricle, the appendix of which lies in contact with the chest wall in the second left interspace. He further holds that the murmur of mitral reflux may be heard in this situation, whilst it is inaudible over the mitral area. At another time, in discussing the basic cardiac murmurs, I hope to show that the murmur described by Dr. Balfour is really an instance of the murmur which is, *par excellence*, most associated with functional conditions—viz., a systolic murmur in the pulmonary artery. I may here merely say in reference to Dr. Balfour's view that, so far as I know, no other observer has met with the occurrence of a systolic mitral murmur audible over the left auricle in the second left intercostal space without an indication of its presence at the apex of the heart, and that it is by no means proved that the left auricular appendix comes to lie in contact with the chest wall.

The cases of Naunyn's murmur reported by Dr. Balfour are in reality, I believe, instances of murmur occurring in a displaced pulmonary artery—displaced to the left owing to changes resulting from dilatation of the right side of the heart. Both Drs. Russell and Byrom Bramwell have noted, in cases of dilatation of the right side of the heart occurring in pernicious anæmia, that a needle passed into the second left intercostal space close to the sternum transfixes the conus arteriosus and did not transfix the pulmonary artery as it does under normal conditions.

I may now briefly refer to some of the conditions under which a mitral systolic murmur is met with independently of any organic disease of the valves.

Most members of the Academy have had, I am sure, under observation cases of cirrhosis of the kidneys where, with the signs of an hypertrophied heart, a systolic murmur of marked intensity could be heard intermittently over the mitral area. This murmur is specially audible under conditions which increase intraventricular pressure, is usually accompanied by signs of widely-spread arterial degeneration, and probably is the index of failing ventricular power. When cases such as I refer to came first under my observation, I believed that organic disease of the mitral valve existed, but *post-mortem* examination of a number of such cases showed that this view was erroneous. In most of these cases there were no appreciable signs of dilatation of the left ventricle, but there was very considerable hypertrophy of its walls and of the myocardium throughout the heart. In some cases the muscular structure had undergone

fatty degeneration; in others a marked fibroid change was noted, in parts beneath the endocardium, in the papillary muscles, and in the walls of the left ventricle. The segments of the mitral valve were smooth, unaltered in texture, or merely presented at their edges slight nodular swellings at the points of attachment of the chordæ tendineæ, obviously not sufficient to impair the competency of the valve. In most cases there was neither absolute nor relative enlargement of the auriculo-ventricular orifice.

There can be no doubt but that great dilatation of the left ventricle is frequently attended by the development of mitral regurgitation. In the case which we ordinarily speak of as a weak and dilated heart, where there are—a flapping character of the heart sounds, increased area of cardiac dulness, and an extremely weak and irregular pulse, mitral reflux intermittingly occurs until, under favourable conditions, the myocardium acquires tone. In aortic patency the occurrence of what I have termed secondary mitral incompetence arises from extreme dilatation of the ventricle, and the condition so induced may be regarded as a safety-valve function which obviates the tendency to fatal syncope from paralytic distension of the ventricle. The conditions just referred to as causing mitral inadequacy may, I believe, be explained in two ways. There is either such an amount of dilatation of the auriculo-ventricular zona tendinosa that the mitral valve becomes incompetent, or reflux is due to a relative shortening of the papillary muscles in consequence of the extreme dilatation of the cavity of the ventricle.

In connection with the occurrence of mitral murmur in dilated heart, I may mention the views entertained as to the influence of certain occupations in producing a mitral regurgitant murmur without organic changes in the mitral valve. Seitz, Forget, Fränzel, and others, state that, in the over-exertions following a campaign or similar laborious occupation, the heart undergoes such a dilatation of its cavities as to render the valves incompetent; and Peacock mentions that miners in Cornwall, from repeated ascent of ladders, suffer from cardiac dilatation and mitral insufficiency.

Lastly, mitral systolic murmur has been developed in certain conditions which favour the development of *ante-mortem* clots in the heart, but murmur from this condition has been more frequently found in connection with the right side of the heart.



## IV. FUNCTIONAL SYSTOLIC OR POSTSYSTOLIC MURMUR.

I now come to the consideration of purely functional systolic mitral murmur—that is, a murmur which depends on temporary and curable conditions, and in which there is no alteration in structure of the mitral valve. I shall not dwell on the opinions held by Bamberger, Flint, and Andrew, that systolic mitral murmur can be produced by irregular vibrations of the valve, by intra-ventricular vibration, or by alterations in the quality of the blood, without the occurrence of mitral regurgitation. Accepting the views of Chauveau and Savart in explanation of the mode of production of *bruit de soufflet*, I believe that in cases where a mitral systolic murmur exists it is an indication of regurgitation; and I agree fully with Dr. Bristowe, who regards it as an axiom “that the existence of a systolic murmur at the apex of the heart is a sure indication of incompetence of one or other of the auriculo-ventricular valves.” It is scarcely conceivable that murmurs having the same acoustic characters, heard over the mitral area, could in one case be due to reflux of blood, in another to irregular tension of membranous structures, such as the valve segments or intra-ventricular tendinous cords.

It has often occurred to me that undue importance may be attached to the existence, *per se*, of a faint bellows murmur, audible over the mitral area, when developed in conditions of great excitement of the circulation. I do not, in any way, mean to imply that the existence of systolic mitral murmur should not lead to the closest investigation into the physical condition of the subject of it. On the contrary, I believe it to be of the utmost importance to determine whether the murmur is due to organic disease, or depends on functional conditions, trivial in character and removable. I have had, within the past few years, in connection with some appointments which I hold, an opportunity of investigating the condition of the heart in young and vigorous adults of both sexes: in one instance in connection with a training college, where admission can be gained only upon a medical certificate of good health; in another, in reporting on the physical state of applicants for admission into a branch of the public service. In most of these cases there is considerable excitement of the circulation of a nervous character; and I have been surprised at the frequency with which a systolic *bruit*, best heard over the mitral area, was developed. In some instances a diffused murmur was heard over

the greater part of the cardiac area, and was clearly pulmonic in origin; but in a certain number the murmur was, as far as I could determine, one of mitral reflux. It was only after repeated examinations, when the great frequency of the pulse had subsided, and that the murmur ceased to be audible, I was satisfied that no organic disease existed. How many persons have been excluded from the public service or rejected for insurance because under conditions of excitement an apex murmur was developed from purely functional causes—causes which I believe are more efficient in producing this murmur than is generally supposed? I know of two members of my own profession, practising in this city, who were prevented entering the army medical service because, under nervous excitement, an apex murmur was developed from purely functional conditions. Both of these gentlemen are, I am happy to say, in good health and perfectly free from any sign of heart disease. I shall add, that it would be of the utmost importance if an extensive series of observations were made and recorded of the sounds of the heart in persons believed to be in a condition of health under various conditions of excitement of the circulation. I would venture to hope that this may be done by some of our brethren in the public services where favourable opportunities for conducting such examinations exist.

In a short paper which is published in the *Dublin Journal of Medical Science*, June, 1873, I ventured to classify the conditions under which a functional mitral murmur is met as follows:—

First,—It may occur, and be constant in duration, in dilatation of the left ventricle associated with hypertrophy, or in simple hypertrophy of the left chamber. In both of these instances advanced degenerative changes of the myocardium will, I believe, be generally found.

Secondly,—Where the auriculo-ventricular orifice becomes so dilated as to render the valves incompetent.

Thirdly,—In adynamic conditions of the system, leading to defective innervation of the heart, as in cases of cerebral disease, and in those cases of low typhus fever recorded by Dr. Stokes, where the first sound at the apex was replaced by a soft blowing murmur.

Fourthly,—In certain neuroses of the heart which produce an irregular action. In this class I would include some cases of heart complication occurring in chorea, and those due to excessive use of tea and tobacco.

And fifthly,—In those cases which have been described by Drs.

Hayden and Da Costa, where the murmur was due to an irritable condition of the heart, with palpitation, or traceable to various conditions of the system, as anæmia, purpura, nervous debility.

Instances where murmur arises from the presence of a clot in the cavity of the ventricle, and where it is due to obstructed pulmonary circulation, are not included in these classes.

Murmurs occurring in course of, or subsequent to, attacks of acute rheumatism and typhoid fever should be added to the above list.

The points of differential diagnosis between functional mitral murmur and that arising from organic disease were enumerated as follows:—

- (a.) The functional murmur is inconstant and variable in intensity.
- (b.) Usually present in the recumbent posture, it generally ceases when the patient sits up.
- (c.) It is loudest not at the apex, but over the body of the left ventricle.
- (d.) There is absence of signs of pulmonary distress, and of accentuation and doubling of the pulmonary second sound.
- (e.) There is no alteration in the size or position of the heart.
- (f.) The radial pulse is variable as to rate and volume.

There is one other condition not mentioned which I consider of importance to note here. The murmur of functional incompetency is not, as a rule, a substitute murmur; a left ventricular first sound is generally audible, and the murmur can be recognised as following the systolic sound, hence its proper designation is "postsystolic murmur"

Most of the conditions referred to in the differential diagnosis of the murmur can be explained by assuming the existence of an atonic condition of the muscular fibres of the left ventricle. The site of maximal intensity of the murmur being slightly above the apex is somewhat difficult to explain. Possibly here the murmur obeys the law of convection, whilst in organic disease of the valves the vibrations are carried towards the apex of the ventricle by the structures in contact with the diseased valve.

Time will not permit of my again discussing the various theories regarding the mode of production of functional regurgitation, if, indeed, any advantage could arise from my doing so. In the paper referred to I ventured to suggest it might result from an irregularity, or want of correspondence, in the action of the sets of fibres of the ventricle which obliterate its cavity and those which close its valve. I shall here only dwell for a moment upon a theory as to the mode of production of the murmur put forward by Dr.

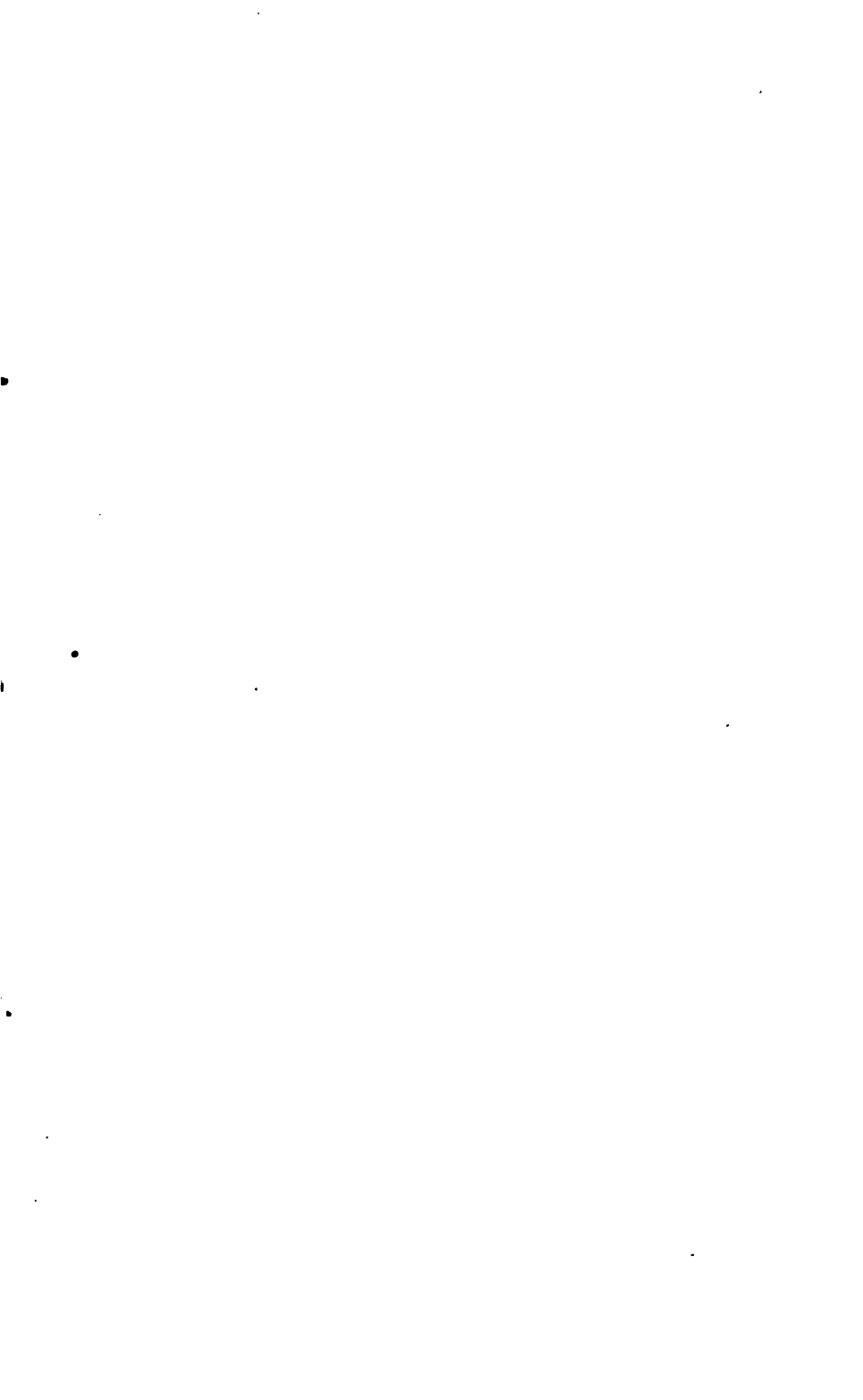
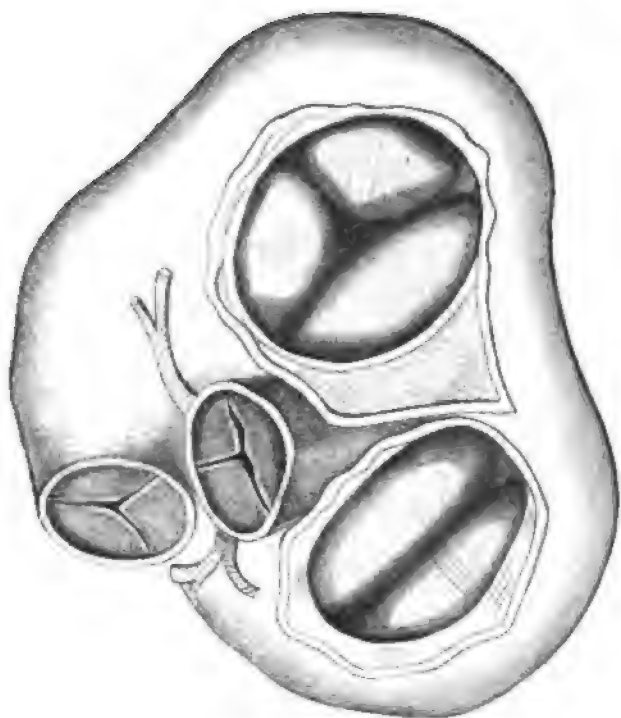


Fig. 8.



Dublin.

Fig. 7.

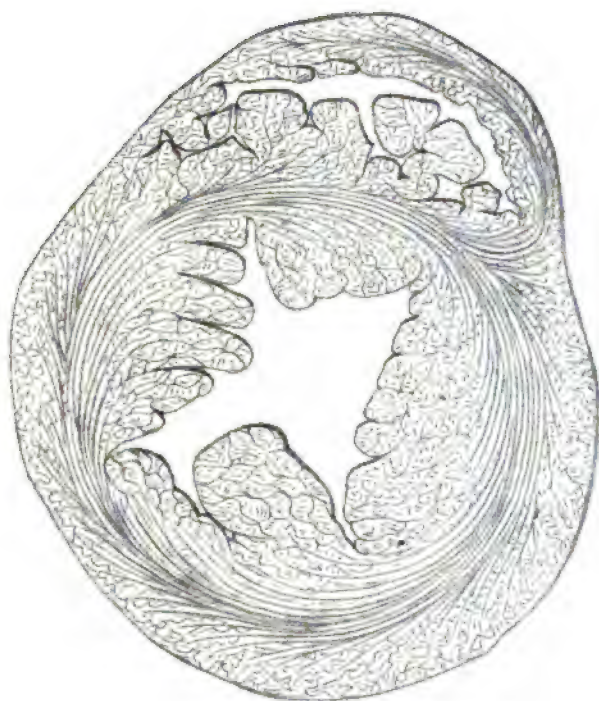


FALCONER.

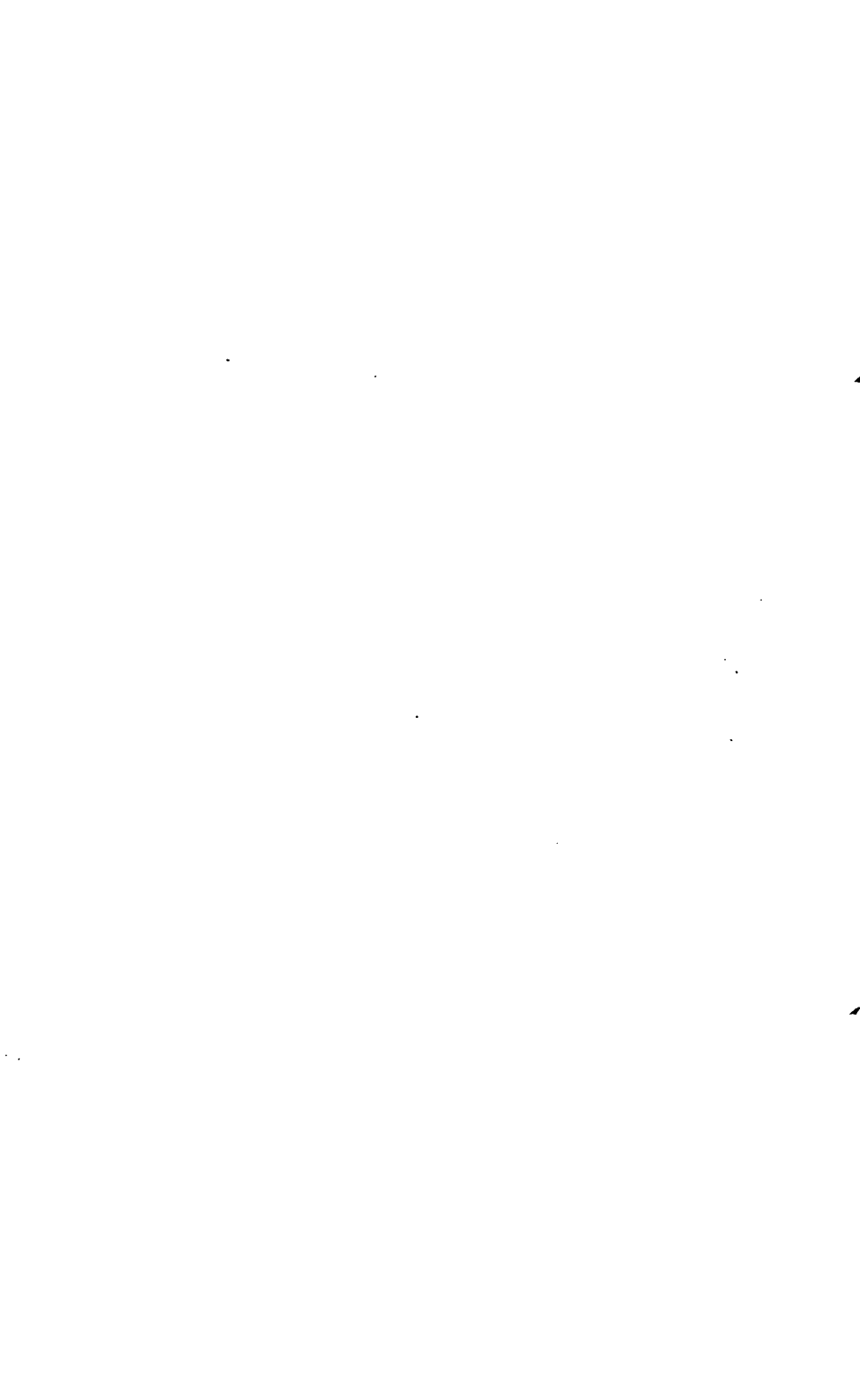




**Fig. 9.**







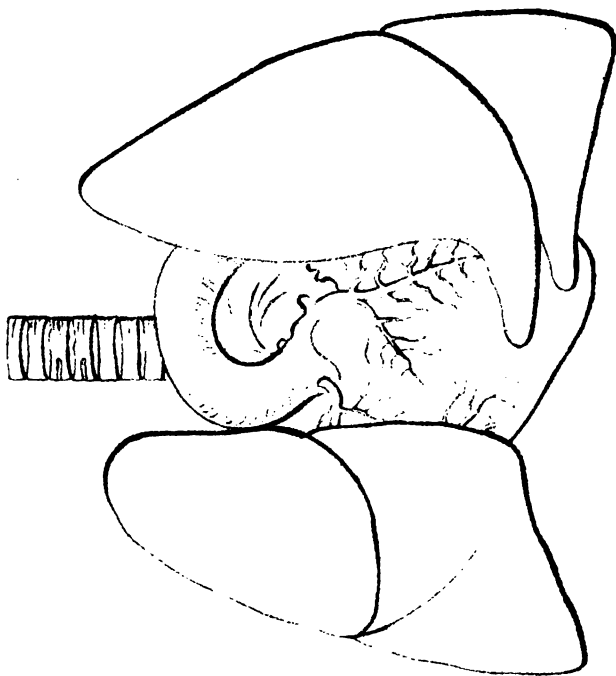
Byrom Bramwell. I do so with some interest, as the view entertained has a certain relation to that urged by myself, but it aims at an explanation of the murmur, at least, remarkable for its simplicity. Dr. Bramwell states that the mitral orifice is surrounded in its posterior two-thirds by the muscular fibres of the ventricle, whilst the anterior third, which is fibrous—I lay special stress on this—is formed by the fibrous continuation of the two posterior aortic sinuses to which the great anterior flap of the mitral valve is attached. The occurrence of mitral incompetency is explained very simply by a want of tone in the muscular fibres of the left ventricle and of the muscular fibres which surround the mitral orifice—the muscular sphincter, as it is termed by Dr. Bramwell. I am afraid the theory of mitral regurgitation, based upon this view of its mechanism, will scarcely bear rigorous examination. No one values more than I do Dr. Bramwell's contributions to the literature of our profession, but it seems to me that in this matter he is clearly at fault. His explanation would appear to be based upon the experiments of Ludwig and Hasse at Leipzig, made in 1880, showing that the size of the auriculo-ventricular orifices are fully one-half smaller in systole than in diastole, as is shown in Figs. 7 and 8. It appears, further, to have been suggested by an interesting *résumé* of Hasse's and Ludwig's observations in a lecture delivered at Cambridge by Dr. Donald MacAlister upon "The Form and Mechanism of the Heart;" but a careful perusal of this lecture does not, in my opinion, afford grounds for the description of the mitral orifice given by Dr. Bramwell. As described by Henle, the left auriculo-ventricular zone is a half ring composed of strong fibrous tissue interposed between the auricle and the ventricle, the ring being completed by the continuity of the anterior flap of the mitral valve with the two posterior aortic valves. This ring forms a perfect line of demarcation between the auricle above and the ventricle below. It has attached to it above the fibres of the auricle, whilst below the fibres of the ventricle are connected with it in the following manner:—The external longitudinal fibres arise from it; pass then downwards until they reach the whorl or vortex at the apex, where some of the fibres pass inwards to form the papillary muscles; the remaining fibres spread out into an inner layer of muscular fasciculi, which are continued up to the fibrous rings at the base of the heart. An important point to note in connection with these fibres is that they arise from the fibrous rings, pass over the external

surface of the heart, and return again to the fibrous rings, internally, either directly or through the intervention of the chordæ tendineæ and valve flaps. Between this external and internal layer of muscular fibres intervenes a set of fibres, the middle layer, which are also attached to the rings at the base, and which pass downwards with increasing degrees of obliquity, until, at a certain distance from the apex, the fibres can be said to pass transversely, and here, no doubt, they assume the form of a sphincter muscle (see Fig. 9). But this arrangement of fibres does not take place in the supra-papillary region of the ventricle. It would, in my mind, be a disadvantage to have a sphincter muscle above the termination of the papillary muscles and forming part of the auriculo-ventricular zone. It would be likely, during its contraction, to interfere with the integrity and smoothness of the aortic ring; and further, there would be no occasion for its existence here, as the small volume of blood which is left in the ventricle after its contraction, and which fills the supra-papillary space (the aortic vestibule of Sharpey), has an important use assigned to it. The change in shape of the auriculo-ventricular rings during systole is obviously due to the combined action of the longitudinal and oblique fibres which are attached to it, and it remains to be proved whether an atonic condition of those fibres, by not sufficiently narrowing the ring, can lead to incompetency of the valve.

There is one fallacy in connection with the detection of an apical murmur that must be carefully guarded against. It occasionally happens that a sound like in acoustic character to *bruit de soufflet* is audible during inspiration corresponding to the systole of the ventricles, and consequently systolic in rhythm. The sound is sometimes heard during expiration, but this is unusual—in fact, the important element in the recognition of this sign lies in the murmur being almost invariably absent during expiration, and it can usually be caused to disappear at the time the patient forcibly expires and ceases to breathe. This murmur is clearly of respiratory origin, and it is produced by displacement of air during the systole of the ventricles in the tongue-like process of the lung which commonly lies over the apex of the heart, which is sometimes developed in a marked degree, and usually emphysematous.\* A drawing, taken from a specimen in the dissecting-room of Trinity

\* My colleague, Dr. M. Boyd, kindly called my attention to a case at present under his care in hospital in which the physical signs corresponded precisely with those mentioned.

Fig. 10.





College, was kindly lent to me by the University anatomist, Dr. Little, and it accurately represents the condition of a lung with an emphysematous-tailed process overlapping the heart, which I exhibited before the Pathological Society in the Session of 1874-75, in which an extra pericardial sound was developed from the movements of the heart against it. Fig. 10 represents the condition referred to.

#### MITRAL BRUIT DE SCIE.

There is but one other murmur developed at the mitral area that I shall refer to, dealing with it with all reserve, as it is one in which my own experience of it is limited to one case, the pathological nature of which was not confirmed by *post-mortem* examination. Some years ago I had under observation in hospital a patient in whom a well-marked *bruit de scie*, identical in character with the murmur developed in most cases of aortic patency, was heard over the mitral area. The murmur was altogether different in acoustic character from the double murmur heard occasionally in mitral stenosis—that is, the presystolic and postdiastolic murmurs. The first murmur was systolic, and was transmitted towards the axilla; the second murmur was strictly diastolic, being exactly synchronous with the second sound. No murmurs were audible over the aortic area; and the cardiac and pulse tracings which were obtained showed no evidence of aortic incompetence. Corresponding to the apex there was a well-marked diastolic impulse of limited extent and expansile character, accompanied by a buzzing thrill and signs of elongation of the heart. Dr. Hayden, who reports the case, in his work on “Diseases of the Heart and Aorta,” says:—“The diastolic murmur was of strictly apex origin; an apex diastolic (not postdiastolic) murmur is one of the rarest phenomena in cardiac acoustics, and can, in my judgment, only be due either to abnormal communication between the aorta or pulmonary artery and one of the ventricles, or to an aneurysm springing from the wall of either ventricle.” Upon this opinion I offer no comment. Personally, I have not met with a case of aneurysm of the heart where the diagnosis was verified by *post-mortem* examination; but I consider, in dealing with the subject of murmurs developed at the mitral area, the views of an observer like Hayden deserve more than a passing notice. I may add, that in a specimen of aneurysm of one of the aortic sinuses opening into the left ventricle, exhibited before the Pathological

Society by Dr. Stokes, an accurate diagnosis of the case had been made by Dr. Hayden from the existence of the murmur now referred to.

I have to thank the members of the Academy for the patient way in which they have listened to a tedious disquisition upon a subject so trite as that which I have just discussed. It would be difficult, indeed, to add much that is fresh and novel to the literature of cardiac disease, though, in some points, I do not think what I have ventured to bring before you is open to the criticism—"that what's true is not new, and what's new is not true."

In reviewing the literature of cardiac disease we have good reason to regard with pride the work that has been done by the Dublin School; and though it is not given to many to rival the genius of Corrigan, the philosophic teaching of Stokes, or the erudition of Hayden, it is open to each one, and incumbent on him, to pay the debt which he owes to his profession, by adding to the general storehouse of facts which help to build up the science of medicine.

ART. XXV.—*On the Quantitative Estimation of Albumin, Urea, and Sugar in Urine.*\* By F. R. CRUISE, M.D., Univ. Dublin; President of the King and Queen's College of Physicians in Ireland, and Consulting Physician to the Mater Misericordiæ Hospital, Mercer's Hospital, and the National Lying-in Hospital, Dublin.

LAST year I brought under the notice of the Academy of Medicine in Ireland some brief observations on the value of the waters of Contrexéville in the treatment of various morbid conditions of the urinary secretion. On the present occasion I intend to supplement those experiences by pointing out simple methods for the quantitative determination of albumin, urea, and sugar in urine.

It is hardly needful for me to premise that the present communication is not intended to be an exhaustive one, but merely a contribution, *from a clinical point of view*, towards a subject the interest of which daily increases.

\* Read before the Medical Section of the Academy of Medicine in Ireland, Friday, December 18, 1885, and Friday, April 30, 1886.

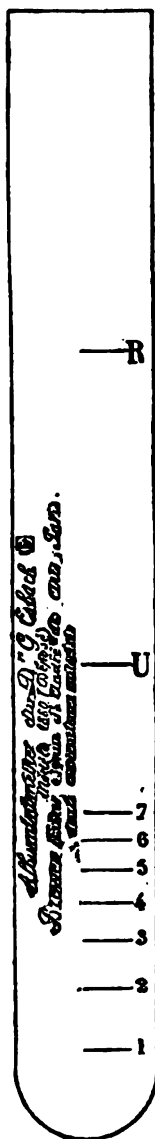
Last autumn I spent a fortnight at Contrexéville, and had the advantage of working daily in the laboratory of Dr. Debout D'Estrées, the accomplished Medical Inspector of that station. There I had the opportunity of extending and confirming my knowledge of the beneficial effects of these waters, especially in reducing albumin and sugar in the urine, and of observing the rapidity and precision with which Dr. Debout D'Estrées verifies the progress of the cases under his observation. Amidst much that I saw to admire in his examination of urine, chemical and microscopical, I was particularly struck with the methods he used for ascertaining the quantity of albumin and sugar in any given specimen, and I propose to describe them now. In addition, I shall detail a very simple and rapid mode of determining the quantity of urea.

It is quite unnecessary for me to allude here to the importance, in any given case of albuminuria, of being able to ascertain with facility the quantities of albumin and of urea present from time to time. The amount of albumin often indicates the extent of renal mischief, especially of the inflammatory type—an element hardly ever quite absent even in chronic organic kidney disease, and frequently playing a very leading part. The quantity of urea is a still more critical item, inasmuch as its presence in sufficient quantity is a fair guarantee of the absence of present danger to life in chronic cases, while its marked deficiency points to imminent risk.

The simple method I am about to describe for albuminimetry is that devised in 1880 by Dr. Esbach, Chief of the Chemical Laboratory at the Hôpital Necker, in Paris. Very few physicians engaged in active practice are able to spare sufficient time to determine the quantity of albumin by the conventional method—namely, boiling a given quantity of the urine so as to precipitate the albumin, then filtering, and finally drying and weighing the deposit. This process occupies several hours of attention, requires suitable apparatus, and not a little manipulative skill. Most of us are habitually contented with the rough and ready plan of boiling the urine in a test tube, then allowing the deposit to settle, and finally guessing the amount from the depth of that deposit. Now, Dr. Esbach's method is nothing more or less than this latter rude device, carried out in a most ingenious and scientific fashion. He takes a glass tube, Fig. 1, of fixed capacity, and graduated according to the



Fig. 1.



results of a carefully-conducted series of experiments. Into this tube the urine in question is introduced up to the letter U, then the re-agent (a solution of picric and citric acids, of fixed strength) is added up to the letter R. The urine and re-agent having been mixed thoroughly the tube is set aside standing for twenty-four hours. At the end of that time the deposit of picrate of albumin has fallen to the bottom in a dense mass or coagulum, and is read off by means of the graduations on the tube, *which give the proportion of albumin in grammes in each litre of the urine under examination.* The time occupied in these manipulations amounts to *about one minute.* It is essential that the urine used should be *acid*; if it is neutral or alkaline acetic acid must be added until it reddens litmus paper.

I may observe that Dr. Esbach's test solution of picric acid serves not only for quantitative analysis of albumin, but also for its detection if present. Thus, if we put a couple of drachms of it into the tube, and add the urine drop by drop (filtered if turbid), if albumin is present a cloud *instantly* appears. Experience has taught me that lithates in the urine are thrown down by the re-agent, and we must be on our guard to avoid deception. In such cases the lithates will be observed in minute dots all down the tube, and the deposit, in place of being a coagulum, will be a thick slimy fluid. Esbach's tubes for albuminimetry, with descriptive pamphlet, formula for re-agent, &c., can be had from Messrs. Brewer, frères, 43 Rue St. André des Arts, Paris. Dr. Veale, in the *British Medical Journal* for May 10th, 1884, gives an excellent paper on this method; and a brief notice of it appears in the *Lancet* of January 23rd, 1886, by Mr. Blomfield.

I have already alluded to the vital importance of being able to ascertain the quantity of urea excreted, especially in the cases of

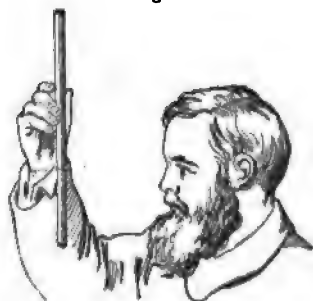
albuminuria which so constantly fall under our observation. An easy process for this determination is that of Dr. Esbach, whose method of albuminimetry I have just described. It is one—and I believe the most facile and perfect—of the many processes by which the urea in a given quantity of urine is decomposed by a solution of hypobromite of sodium, and its amount ascertained by measuring the volume of nitrogen evolved. It is a pleasing duty to remark that this method is really the invention of a distinguished Irish chemist, Dr. Edmund W. Davy. Dr. Davy used the hypochlorite of sodium as the decomposing agent, and at the present day chemists are divided in opinion as to whether the hypochlorites or hypobromites are to be preferred. With both the principle is identical. Dr. Davy published his method in 1854, during the time that I was a pupil in the old Carmichael School of Medicine in North Brunswick-street, where he then lectured. Reference to his original paper (which will be found in the number of the *Philosophical Magazine* for June, 1854) will show how very closely Dr. Esbach has followed his footsteps, even in the apparatus he uses.

Dr. Esbach's method is as follows:—He takes a glass tube, 15 inches long, closed at one extremity, and having the orifice at the other end neatly ground so that it can be effectually stopped by the operator's thumb, previously covered with an India-rubber finger stall. The tube is graduated in cubic centimètres and millimètres, and, corresponding to 140 millimètres, a mark is engraved all round the tube, so as to be visible however it may be held. To use the ureomètre the operator proceeds as follows:—

Fig. 2.



Fig. 3.



First he introduces, by means of a pipette, about eight centimètres of a rather strong solution of hypobromite of sodium (Fig. 2), adding distilled water from a dropping bottle until the

mixture reaches 140 millimètres. Care must be taken to see that the total reads *exactly* at 140 (Fig. 3). Next he takes one cubic centimètre of the urine in question, and having discharged it rapidly into the tube (Fig. 4) closes the latter promptly (Fig. 5), and by a couple of movements of inversion and a good shake, thoroughly mixes the contents. Decomposition sets in at once,

Fig. 4.



Fig. 5.

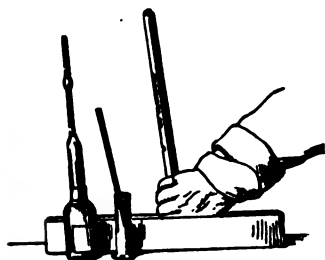


and a considerable froth is developed. This settles quickly into a few large bubbles by holding the tube in the position indicated in Fig. 6, swaying it gently a few times; he then plunges the end of the tube, closed by the thumb, into a water bath, and

Fig. 6.



Fig. 7.



removes the thumb (Fig. 7). The level of the fluid is rapidly lowered by the pressure of the nitrogen evolved. As soon as all is

steady he lowers the tube nearly horizontally, as in Fig. 8, again closes it with his thumb, under water, removes it from the water

Fig. 8.

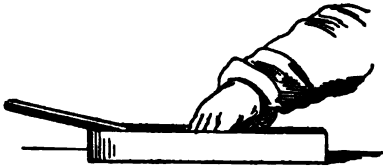


Fig. 9.

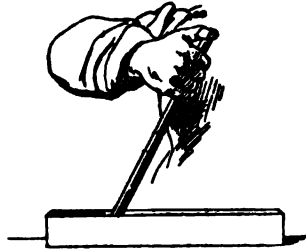
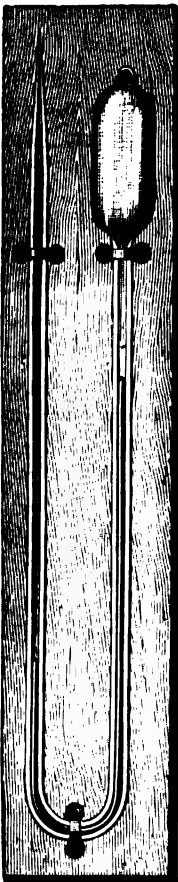


Fig. 10.



bath, as in Fig. 9, and holding it upright, reads off the amount of fluid remaining. This will be less than the 140 millimètres with which he commenced operations, *and the difference between the two amounts gives the volume of nitrogen evolved.*

The next point is to estimate the urea from the volume of nitrogen. If we were left to calculate this, making allowance for the gaseous tension at the moment, we should have a rather troublesome problem to solve; but Dr. Esbach saves us all this, as follows:—*First*, he provides us with a corrective instrument which he terms a Baroscope (Fig. 10). It consists of a bent glass tube, in the elbow of which is a column of mercury, above which on one side is the tube simply closed, on the other side is a small quantity of water, and a thin expanded glass bulb. The working of this instrument is easily seen:—According to the temperature and gaseous tension the mercury is depressed or the reverse, and an index marks the amount. *Secondly*, Dr. Esbach provides us with a set of tables, carefully worked out upon the basis of the foregoing data, which show us the amount of urea, as indicated by the two figures we have—namely, the volume of nitrogen in millimètres, and the indication of the baroscope. For example, if the volume of nitrogen

amounts to 50 cubic millimètres, and the baroscope points to 72, we find at a glance that the amount of urea is 13·9 grammes per litre. Taking, for a working estimate, each gramme as 15 grains, and the litre as 35 oz., we find that such an urine contains 205 grains per litre—nearly 6 grains per oz. Supposing, then, that the patient passes an average of 60 oz. of urine per day, he is eliminating just 360 grains of urea daily—quite a fair amount under ordinary circumstances. If, on the other hand, the urea falls to half or quarter of the above quantity, we have good reason to fear toxic symptoms. When the urine is albuminous, it is necessary to boil and filter it to get rid of the albumin, which would lead to an error, and a difficulty in reading off the quantities after decomposition, owing to the persistent froth it causes.

All these steps take some time to describe, but once learned are very quickly gone through. The average time needed for the quantitative determination of urea by this method is *five minutes*. The needful apparatus, with descriptive pamphlet, formulæ, tables, &c., may be had from Messrs. Brewer, frères, of 43 Rue St. André des Arts, Paris.

We shall now discuss the quantitative determination of sugar in urine, and, I trust, find that the process can be accomplished by a method at once easy and rapid in use, and satisfactory in result. It is needless for me to dwell upon the importance of quantitative analysis of saccharine urine, because we all know that without such information the progress of a case of diabetes cannot be properly watched, or the effect of treatment—medicinal or dietetic—estimated at its correct value.

I need not delay upon the subject of *qualitative* analysis—many excellent tests are familiar to every student of medicine; therefore I shall pass directly to the subject of the *quantitative* analysis of diabetic urine.

The principal methods by which we are able to determine the amount of sugar in urine are:—

- I. The fermentation test.
- II. Volumetric analysis.
- III. Polariscopy.

#### 1.—FERMENTATION TEST.

The fermentation test, at the best, is but a rude device, and as it occupies many hours, is unsuited to the exigencies of every-day practice.

## II.—VOLUMETRIC ANALYSIS.

The method by volumetric analysis, although far more rapid and precise, is open to grave objections. It occupies a considerable time—rarely less than an hour. It requires a well-appointed laboratory, and special skill on the part of the operator. Moreover, it is liable to many considerable errors, amongst which I may mention the principal—

(a) The copper test-solutions are liable to deterioration, by keeping, to such an extent that they must be tested carefully before each analysis. If long kept they may throw down the copper on simple boiling, even without the presence of sugar, leading to an unfounded belief that it is present when it actually does not exist. Hence, if previous testing of the solution be omitted, it is quite possible to reach a false conclusion as to the existence of diabetes. I need not point out how serious an error this might be, supposing, for example, that we were examining the urine of a patient presenting himself for life insurance, &c.

(b) The point of decolorisation, or reduction, is not easily determined, except by a very practised eye, and is not satisfactorily reached unless in daylight.

(c) When we have to deal with strongly saccharine urine it is scarcely possible to avoid errors, which, being multiplied in proportion to the relation the quantity of urine used bears to the total excretion, become very considerable indeed. To make this point clear let me give an example. Supposing that the urine in question contains 20 grains of sugar per oz., or  $2\frac{1}{2}$  grains per drachm, and that we use the ordinary Fehling's solution, of which 26 cc. are decolorised by 2 grains of sugar, an error of half a drachm in excess added to reach the point of decolorisation, leads to an error of  $1\frac{1}{4}$  grains in the operation. Now, if the patient excretes 100 oz. of urine in 24 hours—a very usual quantity in diabetes—the error of the half drachm causes a huge one in the total. The half drachm is the  $\frac{1}{1600}$ th part of the total urine, and hence the initial error of  $1\frac{1}{4}$  grains would lead to a final error of 2,000 grains. If the excess of quantity amounted only to 15 drops, or quarter of a drachm, still the final error would be 1,000 grains, and so on in proportion. We can, to a certain extent, avoid such an error by conducting the latter stages of the analysis *very slowly*, adding the urine guttatum and watching the effect, but this process, I need not observe, involves much delay. Thus, it is evident that volumetric

analysis is tedious, requires much skill, and is liable to serious error. It may be urged, in extenuation, that any strongly saccharine urine may be previously diluted to a fixed extent, and allowance made at the final computation. This, however, involves extra calculation and delay.

(d) Chemists are not agreed as to whether other substances besides sugar, occurring in urine, do not exercise a reducing power on the copper solution, and thus further error comes into existence. For all these reasons it seems obvious that volumetric analysis is unsuited to the busy practitioner, who, to obtain reliable information, must confide the specimen to an expert.

### III.—POLARISCOPY.

Is there, then, no method of estimating sugar in urine—simple in application and satisfactory in result, which may be resorted to for daily use?

I learned one from Dr. Debout D'Estrées while I was at Contrexéville last autumn, and I shall describe it now. It consists in the use of a polariscope, specially constructed for the purpose, by the aid of which the estimation of sugar can be accomplished with approximate accuracy in less than ten minutes and without the need of any special skill. After an extended trial during the last six months I am quite satisfied with this method.

A vast number of cases of diabetes come to Contrexéville, on account of the remarkable effect of its waters in this disease, and Dr. Debout D'Estrées makes his analysis as follows:—

*First*—He ascertains the presence of sugar by a qualitative test, say by boiling the urine with caustic potash; and

*Secondly*—Having discovered its presence, he then takes about an ounce of the urine and, after decolorising it by the addition of solution of acetate of lead and filtering, he places a portion of it in the polariscope and reads off the quantity of sugar at once.

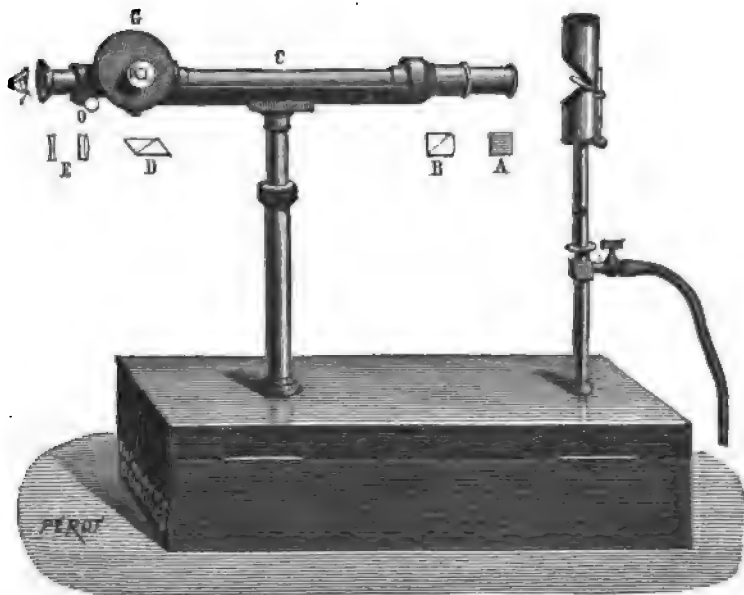
Dr. Debout D'Estrées uses Laurent's instrument. I prefer the Yvon-Duboscq diabétomètre, on account of its simplicity and moderate price. I can scarcely bear higher testimony in its favour than by mentioning the fact that my friend Dr. Tichborne, Chemist to the Apothecaries' Hall of Ireland, is satisfied with its performance. Mr. Stephen Yeates, our well-known eminent scientist, is of the same opinion.

Let me say a few words about the Yvon-Duboscq diabétomètre. The fundamental principle upon which the polariscope depends, as

a means for determining the existence and amount of sugar in a given solution, is the well-known fact that a saccharine fluid always *rotates* polarised light, and that the amount of rotation is in *exact proportion* to the density of the solution. With regard to the present instrument it would be out of place to enter here into minute details of the principles on which it is constructed, and its complete exposition would occupy far too long a time. Suffice it to say that it belongs to the class of "half-shade" polariscopes (polarimètre à pénombre), the first of which was, I believe, constructed about 1860 under the direction of the Rev. Professor Jellett, now Provost of Trinity College, Dublin.\*

In these instruments the mechanism for sensitiveness is arranged to produce a circular field of vision, divided into halves, which in certain positions of the analysing Nicol prism are unequally illuminated, but in one particular position exhibit an uniformly faint shade. This latter position, which admits of being fixed with great accuracy, is taken as the point of reference, and to prevent confusion of colours a monochromatic light is used.

Fig. 11.



\* Those interested in the subject may consult with advantage Landolt's Handbook of the Polariscopes. English translation published by Macmillan & Co., London. 1882.



The accompanying woodcut, Fig. 11, with a brief explanation, will render clear all that is needful for the successful manipulation of this diabétomètre.

The light emanating from a monochromatic yellow flame, obtained by burning a piece of salt in a Bunsen jet, at first passes through a chamber (A) filled with a weak solution of bichromate of potassium, and then traverses the half-shade polariser (B). It continues its course through the tube (C), which holds the liquid under examination, and passing thence is received by the analysing Nicol prism (D), and finally reaches the observer's eye after passing a Galilean eye-piece (E) placed so as to render the vision distinct.

The Nicol prism is secured in a movable collar, the angular movement of which is measurable. For this purpose the above-mentioned collar has a cogged circumference, which is operated on by a tangential endless screw. The stem of this screw (F) has attached to it a circular disc (G) marked with divisions, each of which is experimentally adjusted to represent one gramme of sugar for each litre of the urine under examination in the tube (C). Independent of the screwhead (F) and disc (G), the Nicol prism can be moved as required by the regulator (O), so as to attain the point of reference while the disc (G) is fixed at zero.

To use the diabétomètre the operator first adjusts the point of reference by allowing the polarised light to pass through the tube (C), previously filled with distilled water, and then rotates the Nicol prism by means of the regulator (O) until the field of illumination is perfectly evenly lighted up, as in Fig. 13.

Fig. 12.



Fig. 13.



Fig. 14.



Now he substitutes for the tube of water one containing the saccharine urine in question. Immediately on doing so he finds one-half of the field of vision darkened, as in Fig. 14, and to rectify this he must turn the disc (G) until the two halves of the illuminated disc are *perfectly evenly lighted*. This can be done with the greatest accuracy, as, if the disc is turned the least degree too far, the other half becomes darkened, as in Fig. 12. A little practice enables the operator to do this within a quarter of one of the divisions of the disc. The new position of the disc

(which indicates the rotation of the Nicol prism) points out the amount of that rotation—or, in other words, records the amount of the sugar in the urine, each division representing one gramme per litre.

I do not mean to assert that the quantitative examination of diabetic urine by the polariscope is absolutely free from the possibility of error. If the quantity of sugar is very minute error may occur to a small extent, and if the tube containing it is too short the accuracy is further jeopardised, but certainly with a well-constructed instrument these errors are very trivial compared with those which beset volumetric analysis.

Certain precautions are needful with the Yvon-Duboscq diabétomètre. The urine must be perfectly clear, and to render it so it is needful to decolorise it by agitation with animal charcoal, or by adding some solution of acetate of lead and then filtering. I prefer the acetate of lead for several reasons. The animal charcoal must be perfectly fresh, and even then it destroys a fraction of the sugar. The acetate of lead solution has only one drawback—namely, that it dilutes the urine, and allowance must be made for this at the final computation. To lessen this difficulty I use a burette marked with a graduation at 50 centimètres and again at 55 centimètres. I fill up to 50 cc. with urine, and then add 5 cc. of liquor plumbi. This gives a definite proportion of one-tenth, which must be added to the total sugar indicated—for example, if the amount indicated is 50 grammes, it is necessary to read it at 55, and so on in proportion.

The result of my experience is, that for the adjusting of the diabétomètre, clearing of the urine, and its examination, a period of *ten minutes* amply suffices.

The Yvon-Duboscq diabétomètre, with full instructions and all needful appliances, can be had from the makers, Messrs. T. & A. Duboscq, 11 Rue de Fossés, St. Jacques, Paris.

In conclusion, I venture to recommend to all busy practitioners who wish to make fairly accurate, but easy, quantitative analyses of albumin, urea, and sugar in urine, the methods which I have now described, and which I brought under the notice of the Academy of Medicine in Ireland during the present session of 1885-86.

**ART. XXVI.—*Clinical Notes on a Case of Acute Lichen Planus.*\***

By J. MAGEE FINNY, M.D. Dubl. ; Fellow and Vice-President, King and Queen's College of Physicians in Ireland ; King's Professor of the Practice of Medicine ; Clinical Physician to Sir Patrick Dun's Hospital.

CASES of lichen planus are of sufficiently rare occurrence in this country as to make me think that the recital of one such case will prove not lacking in interest. The very acute course the disease ran, its very extensive manifestations over almost the entire body of the patient, the head and face alone being exempted, and its rapid and permanent cure, are points in its clinical history which may be expected to enhance that interest.

I have paid considerable attention to the study and treatment of diseases of the skin, and for the last twelve years have, one day in each week, had the advantage of seeing, at the extern department of the hospitals to which I have been physician, a great variety and number of skin diseases, but I never met with so exaggerated or so marked a case as that which I now bring under your notice ; and I think my experience and observation agree, probably, with those of other physicians in the United Kingdom :—

Mrs. H., aged fifty-three, a mother of six children, and of previously healthy medical history, consulted me, on August 29, 1885, for an extremely itchy skin disease, from which she had been suffering for the preceding six weeks. Menstruation had ceased eighteen months before, without trouble, and, except for an irritable rash on the neck twelve months ago, which was seemingly of an eczematous nature, she had never had any skin affection. She was a stout, pasty-looking woman, had a "bad stomach," and occasionally had suffered from rheumatic pains in her shoulders. Her occupation was keeping a lodging-house, and she lived comfortably ; but latterly she seems to have been easily irritated and worried by little things, and to have unduly felt the burden of every-day life. She has been easily tired for some months, and, what is a notable fact, on two occasions, while out in the street, her bowels suddenly acted, although they had moved naturally in the morning.

The "skin disease" for which she consulted me showed itself at first in isolated areas on the legs, below the knees. Later on, it came out on the flexor aspect of the arms and forearms, under the knees, and particularly over the vastus internus, and then on the dorsum of the feet, down towards the toes. Subsequently, while under observation, the rash

\* Read before the Medical Section of the Academy of Medicine in Ireland, Friday, April 30, 1886.

extended over the whole back (being very thick over the loins), lower part of abdomen, nates and thighs, and on the upper extremities, the front of the wrists, and the dorsum of the hands and fingers. The palms and soles were not exempted, and, although the eruption was scanty in these localities, it was very evident along the fibular side of the sole and over the inner eminence of the palm. At the acme of the disease, which may be said to have been reached about the third week in September, no part of the body was free of the eruption, except the face, scalp, and neck. The photograph [exhibited], taken October 10th, faithfully depicts the millions of papules which constituted the eruption.

The colour of the eruption was at once striking, being, when fully developed, of a dull-red or purplish-red, not unlike a syphilide. It consisted of numerous papules, with a flat surface abruptly raised above the adjoining skin. The papules varied in size from a split pea to a three-penny piece (2-6 lines), and were smallest on the fingers and toes, and largest on the thighs, nates, and back. They varied also in consistence, being just perceptible to the finger on the backs of the hands, while they were large, thick, and buttony over the back and loins, and conveyed to the hand the horny hard feel of the variolous pustule when dried up in the third week of smallpox, and produced so much discomfort to the patient that lying on the back was avoided.

The papule wherever situated had the same characteristics. It was solid, abruptly raised, round or polygonal in outline, with sharply-defined edges; the surface was flat, smooth, with a waxy or glazy appearance. Many of the papules were slightly depressed or quasi-umbilicated, and to some a slight filmy desquamation was attached.

The central pitting was not universal, nor did it correspond to the passage of a hair. As Crocker<sup>a</sup> has shown, it is probable that the sweat-ducts, rather than the hair follicles, determine the situation of the papules. The papules were isolated, and even in the large patches on the back, knees, &c., they seemed to maintain their individuality, though by crowding they touched each other. In these *plaques* the skin had a remarkable mapped-out or tessellated appearance, and the increased pigmentation lay rather between than throughout the teeming papules.

During the acute period of the attack, and up to Oct. 10th, there was considerable induration of the skin, due to infiltration, so that the movements of the joints were much interfered with.

The itching was of a most aggravated character, with much irritation and tingling, and equalled the pruritus of scabies and gouty eczema combined. Accompanied with it, and, possibly, consequent thereon, there was much nervous depression. The patient was in very low spirits, with constant sighing and almost complete loss of appetite and

<sup>a</sup> Lancet. Vol. I. 1881.

sleep—in fact, for many nights hypnotics and narcotics were required to secure sleep.

Emaciation was shown before long, and was out of proportion to the length of illness and confinement to bed. The diagnosis was not very difficult, when the whole features of the case were considered, although they did not conform to the descriptions of either *L. Planus* or *L. Ruber* as given by Hebra.

The treatment consisted in alkaline and size baths, sedative lotions, and the internal administration of liq. arsenicalis. In the beginning I employed pilocarpine hypodermically at intervals. To this I was led by the idea that free diaphoresis would relieve the infiltration and thickening of the skin. The patient invariably expressed herself relieved by the sweating which  $\frac{1}{8}$ — $\frac{1}{4}$  gr. readily produced, and to it she attributed most benefit. Within the last few days I find in von Ziemssen's "Handbook of Skin Disease" that good resulted from its use by Dr. Köbner. What seemed, however, to afford more relief to the pruritus than anything else, including sedative lotions, was warm baths and the use of a lotion containing carbolic acid and liq. carbonis detergens.

I made a tentative though full treatment of chrysophanic acid to the right leg and foot, but I am bound to say that I could not see that it was of the slightest use.

The administration of Fowler's solution of arsenic was commenced in 5m. doses on Sept. 12th, and the dose was gradually increased up to 13 minims three times a day, when it produced its physiological effects on the mucous membranes of the eyes, stomach, and bowels. It was then intermitted, and after a few days resumed in smaller doses. It was, however, persevered in till the end of October, a dose of 6 minims being well tolerated. Improvement was very doubtful for the first fortnight, but it was then observed that no new spots made their appearance, while a diminution in the severity of the itching and an absorption of the infiltration set in.

On 7th Nov. not a papule remained, though the pigmentation and stains were seemingly unchanged, so that the skin had, as the patient aptly expressed it, an appearance like a "speckled turkey egg." These stains existed for some months subsequently.

When the patient went from under my care she expressed herself as feeling better in general health and spirits than she had felt for several months before the lichen appeared.

The foregoing case represents a very severe, though typical, aspect of diffuse lichen planus. Many cases are of a very limited area, such as on the glans penis,\* and its course may be very chronic or acute.

\* *Vide* two cases recorded by Dr. Bulkeley. *Arch. Dermatology*. Vol. VII., pp. 185, 392.

Erasmus Wilson first described the disease as a distinct form of lichen in 1869, and looked on it as one of great rarity. He met with but fifty instances among ten thousand miscellaneous cases of skin disease. It is closely allied to, if not identical with, the lichen ruber of Hebra, as many observers have noted these two conditions to have followed one upon the other.

My experience can add no facts on this question. It is, however, a matter for regret that more accuracy of description is not observed in the rather indiscriminate use, by even recent writers, of the terms lichen ruber and lichen planus.

The ætiology of lichen planus is a subject of some difficulty, and this, doubtless, is intimately connected with and due to the imperfect knowledge we have of its pathology. Is it a disease of a localised inflammatory nature, or is it a manifestation of a general disorder?

The following may be said to represent the more recent anatomico-pathological facts of this disease:—

I. Crocker<sup>a</sup> considers the anatomical process at the onset to be a superficial inflammation independent of the hair follicles, and he distinguishes two varieties—

(a) Connected with the blood-vessels.

(b) Due to proliferation of cells of the rete.

The sweat-ducts often appear to determine the localisation of the papules.

II. Neumann found the chief changes to be in the upper layers of the cutis, and to consist in thickening of the layer of horny cells and increase of the rete.

III. Dr. A. Weyl (the writer in von Ziemssen's "Handbook of Skin Disease," 1885, just printed) considers that the process begins as a perivascular proliferation of the vessels of the upper layers of the cutis, and is followed by an infiltration of cells into the papillæ following the course of the nerves.

The general consensus of opinion seems, however, to be in favour of the view that the nervous system is primarily at fault.

Dr. Tilbury Fox,<sup>b</sup> and more recently Dr. T. Colcott Fox,<sup>c</sup> considers that the local disease is not of an inflammatory nature, but is a simple chronic neuroparalytic hyperæmia in localised areas with its consequent effects.

<sup>a</sup> Lancet. 1881.

<sup>b</sup> Skin Diseases. 3rd Edition. 1878.

<sup>c</sup> Brit. Med. Journal. 1879. Vol. II., p. 292.

There is in the majority of cases\* a history of debility arising from overwork, nervous depression, and improper nourishment, and the symptoms which accompany it are chiefly referable to nervous exhaustion and prostration. Moreover, the symmetry of the eruption and the benefit which accrues from a nerve tonic like arsenic point to its neurotic origin.

As to its treatment, arsenic may be looked upon in the light of a specific for lichen planus, having the authority and weight of the sanction of E. Wilson and J. Hutchinson. Duhring also strongly recommends it, and lays stress upon its use being persevered in; and a recent writer, Dr. Lavergne,<sup>b</sup> urges the necessity of administering it in considerable doses. Köbner<sup>c</sup> records a case of rapid cure of lichen ruber by the hypodermic injection of liq. arsenicalis (diluted) after it had failed by the mouth, the itching having yielded to the first few injections.

On the other hand, while there is so great an array of cases in which arsenic may fairly be credited with good, it is right to note that Dr. Tilbury Fox<sup>d</sup> (in 1873) states, "With regard to arsenic, I can only say that it has always made my cases worse." R. W. Taylor<sup>e</sup> recorded in 1874 four cases of severe acute lichen planus, a disease of very great rarity in America, in which alkaline baths and tinct. saponis  $\bar{c}$  pice of Hebra produced a cure in four to six weeks.

Recently Dr. George Thin detailed<sup>f</sup> six cases of lichen ruber which had been cured by Prof. Unna<sup>g</sup> by external means alone, discarding the use of arsenic altogether. To this series Dr. G. Thin adds another instance in his own practice where by soft soap and white precipitate ointment he cured a case of lichen planus of the glans penis.

Unna's prescription is—

Ung. zinci benzoat.	-	500
Acidi carbolici	-	20
Hydrarg. bichlorid.	-	0.5-1

and needs care in its use over a large surface, as carbolie acid shows itself in the urine in three to four days. The cases this

\* Duhring. 3rd edition.

<sup>b</sup> Journ. Cut. and Ven. Dis. Vol. II., p. 303. 1884.

<sup>c</sup> Deut. med. Wochenschrift. 1881. VII., p. 3.

<sup>d</sup> Loc. cit.

<sup>e</sup> Archives of Dermatology. 1874. P. 36.

<sup>f</sup> Brit. Med. Jour. P. 425. 1885.

<sup>g</sup> Monatschrift für prakt. Dermatolog. 1882. No. 1.

treatment is said to be specially suited for are those in which great depression exists and much itching, and Unna claims for it both the speediest means of procuring alleviation to the pruritus and of producing a cure.

Dr. Thin, commenting on this treatment, remarks that this subject is one of more than therapeutic value as showing that lichen ruber is more of a local disease than it is usually considered.

In my case both carbolic lotion and the internal administration of arsenic were employed; but I think the arsenic deserved the full credit for its cure.

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ART. XXVII.—*Hæmophilia: its Causes, Symptoms, and Treatment, with an Illustrative Case.*<sup>a</sup> By JOHN M'CAW, M.D. Q.U.I.; L.R.C.P. and L.R.C.S. Ed.; Physician to the Belfast Hospital for Sick Children.

**HÆMOPHILIA** is a congenital tendency to bleeding which manifests itself shortly after birth and lasts the life of the patient.

Hæmorrhage occurs spontaneously, or upon slight provocation, and usually can be arrested only with difficulty. It has also been noticed that the subjects of this diathesis exhibit a tendency to obstinate and painful swellings of the larger joints.

*Causation.*—If not invariably hereditary, it shows a strong tendency to hereditary transmission. It is seen more frequently in the male than in the female offspring, and it has been noticed as a curious fact that the transmission of the tendency is seen more commonly in cases where the parent of the patient, though sprung from a family of bleeders, is individually free from the disease. In the large majority of cases the inheritance is derived from the mother. The disease is found in all countries and in all conditions of life, but the Hebrew race is said to be particularly liable to it.

*Morbid Anatomy.*—*Post-mortem* examinations have revealed little to explain the nature of the affection. No change has been found in the blood, and only in some cases is a partial fatty degeneration of the lining membrane of the vessels found, which is probably due to the extreme anæmia seen in such cases. Large extravasations of blood may be found, and Sir W. Jenner has recorded such a case, where an enormous extravasation was found underneath the fascia of the thigh. The swelling of the joints seem to be due to effusion of blood into the articulations.

<sup>a</sup> Read before the Ulster Medical Society, January 13, 1886.



*Symptoms.*—It is rarely before the end of the first year that any sign of the disease is noticed, and it is not till the child is making efforts to crawl or walk, and thereby becomes exposed to injuries from falls, &c., that his constitutional peculiarity can be recognised. In the lowest degree it may show itself merely in the form of ecchymoses in the skin, and in a higher grade there is seen spontaneous hæmorrhage from the mucous membranes. In the most pronounced forms a tendency to every form of hæmorrhage is observed, in which the mucous membranes may pour out blood without obvious cause; slight injuries may give rise to copious extravasations; or obstinate and painful swellings may attack the joints. Hæmorrhage usually occurs at a time when the patient seems in unusually good health. The bleeding may be preceded by signs of excitement or irritability of temper, and it is said there is actual sharpening of the senses of sight and hearing.

In case the bleeding is spontaneous, it is usually from the nose, but may be seen to come from the inside of the cheeks and lips or gums; and as a rule the younger the child the more likely it is to come from the nose or mouth, and it is only towards puberty that hæmatemesis and melæna become common. Renal hæmorrhage at any age is rare. Not infrequently bleeding from one part is quickly followed by a similar effusion from another, until the patient dies worn out by the constant drain; and when bleeding from one source alone proves fatal it is from the nose usually. In addition to the spontaneous form, it has been noticed that slight wounds or blows may produce copious effusions, and little cuts and scratches bleed obstinately, so that in such cases the scratch of a pin or the extraction of a tooth may induce most obstinate or even fatal hæmorrhage. In all cases the source of the bleeding is capillary, the hæmorrhage occurring as a constant oozing; or in other cases, circumscribed collections of blood may be noticed forming tumours of various sizes. One of the most curious features of the disease in the highest grade is the joint affections; the articulations attacked are usually the larger ones, the knee being the commonest. The joint becomes swollen and tender, is accompanied by pain, which is increased by movement, and there is rise of temperature.

Fluctuation may be detected, and the effusion is most commonly the result of articular hæmorrhage, which may be either the result of a trifling blow or spontaneous.

*Diagnosis.*—In severe cases of hæmorrhagic purpura the symp-

toms may simulate very closely those just described. In such the point to be chiefly relied upon is the hereditary tendency present in hæmophilia and absent in purpura.

*Prognosis.*—Out of 152 boys Grandidier found that only 19 attained the age of twenty-one, and that more than half the number died before the seventh year. Hæmorrhage after the extraction of a tooth is most dangerous, and obstinate epistaxis comes next, so that to these two are to be attributed a large number of deaths.

Children are said rarely to die from a first bleeding, and one profuse gush is to be looked upon as more favourable than a slow and persistent oozing.

*Treatment.*—Protect from all injuries and avoid particularly the extraction of a tooth. Constipation is very injurious. Diet should consist more of white-meat and fish than of the stronger varieties, with a fair supply of fresh vegetables. In case of premonitory symptoms enjoin perfect rest, give a mercurial purge, followed by a saline. Regular exercise in the open air should be enforced, but boisterous games, such as football, cricket, &c., had better be avoided. As regards medicinal treatment, ergot, ergotine hypodermically, and hazeline, have all been highly spoken of and used with apparent benefit in certain cases. Chlorate of potassium and iron may be tried, and for external forms the use of puff-ball should not be overlooked. Flannel should be worn next the skin, together with sufficiently warm clothes, in order that the cutaneous circulation may be well maintained.

The following case came under my notice while on duty as attending Physician at the Children's Hospital:—

*CASE.*—J. S. M'I., aged eight years, of Belfast. Is a tall boy for his years, of light, sandy complexion, with dark brown eyes.

*History.*—Was admitted into the Queen-street Hospital for Sick Children on March 3rd, 1885, suffering from epistaxis of eight days' duration, and I got the following history from his mother, an intelligent woman:—She first noticed a tendency to bleeding when he was fifteen months old, at which time, while trying to run after his father, he fell and knocked his forehead. Over the situation of the blow there occurred a large ecchymosis. She took him to a doctor, and he opened it to let out the bruised blood. He bled profusely from this puncture, and it was with very great difficulty they could get the hæmorrhage stopped. In his second year he fell again, this time on his chin, and nipped his tongue. He bled from this for eight days and eight nights, and it was

only when the child was exhausted that it stopped. Five months after this he fell again—this time on his knee. The joint swelled to a great size and turned quite black, and she says he was confined to bed with it, and the doctor gave her a lotion for it, which she kept applied. Six months after this he knocked the same knee; the same condition ensued, and the same train of symptoms followed. One year after this he cut the index finger of his left hand, and though everything they could think of was done, it bled for six or seven days. Two years after this he fell and knocked his right elbow, and it went through the same processes of swelling and blackening as did the knee. Three months after this his nose began to bleed spontaneously, and only ceased after it had oozed for six days, and when the child was exhausted. Six months after this one of his incisor teeth became loose and he plucked it out, the consequence being he bled from the alveolus for seven days. About nine months after this his nose again began to bleed spontaneously, and had been doing so more or less for the previous week, when he came under my notice at the hospital.

*Family History.*—A maternal uncle had the same tendency when young, but seemed to grow out of it. A brother of his grandmother was similarly affected, and when five years of age he fell on his lip and cut it, from which he bled to death.

*Treatment.*—I gave this boy hazeline, ℥ ii. every hour, and in the evening of the day he was admitted the bleeding ceased. It recurred slightly through the night, and again in the morning. On March 5th he had been very well all day; however, in the evening at 8 20 I was sent for, as he was very ill. I went and found the following condition of matters:—He was suffering acute pain on the left side, below and behind the nipple three inches; the pain radiated round towards the back; temperature,  $105\cdot2^{\circ}$ ; is very restless; percussion note normal; auscultation reveals coarse crepitation on both inspiration and expiration and some mucous râles. I ordered a turpentine stupe to the side and the following mixture:—℞. Tr. aconiti, ℥ ss.; spt. æth. nit., ℥ v.; vin. ipecac., ℥ v.; aq. am. acet., ℥ xx.; syr., ʒss.; aqua ad. ʒi.; this dose every hour till temperature and pulse fall to  $100^{\circ}$  and 90 respectively, and then every three hours. Next morning I found him much better; temperature  $99\cdot6^{\circ}$ , pulse 90 or so, and pain almost gone; crepitations almost gone; nothing to be heard except a few mucous râles.

His mother came the same day and took him home.

March 11th, four days after he was taken out, his nose again bled spontaneously for nearly two days. Mother says the medicine—viz., the hazeline—is of great service. As the mother did not return for some time I sent to make inquiries about her, but unfortunately she had changed her residence and could not be followed. From that time I have heard nothing of this interesting case, a circumstance I very much regret.

## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

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1. *Health Resorts at Home and Abroad.* By M. CHARTERIS, M.D.; Professor of Therapeutics and Materia Medica, Glasgow University. London: J. & A. Churchill. 1885. 8vo. Pp. 156.
2. *Bath, Contrexéville, and the Lime Sulphated Waters, with their Use in Medicine.* By JOHN MACPHERSON, M.D.; Inspector-General of Hospitals (retired). London: H. K. Lewis. 1886. 8vo. Pp. 54.
3. *The Saline Waters of Leamington, with observations on the Climate.* By FRANCIS WILLIAM SMITH, M.D. & B. Ch. Second Edition. London: H. K. Lewis. 1885. 8vo. Pp. 96.

1. OF the three small books, or *brochures*, at present before us, the first, from the pen of Dr. M. Charteris, is much the most ambitious and comprehensive. The work opens with a general classification of mineral waters and health resorts, and subsequently treats in detail of the principal health resorts at home—that is, in Great Britain, for as usual in books of the kind, Ireland is completely ignored<sup>a</sup>—and abroad; health resorts at high altitudes; and sea voyages, with hints as to the prevention and cure of sea-sickness (pp. 73 and 74). The closing portion of the book includes a table for the conversion of the different thermometric scales, an excellent alphabetical list of health resorts, a section on the “metric system” of weights and measures, a glossary of the preparations and doses of the chief drugs included in the Austro-German Pharmacopœia, with their analogues and equivalents taken from the British Pharmacopœia; and, lastly, a short account of the leading routes from London to the more important continental baths and health resorts. There are two indexes—the first, a therapeutical, and the second,

<sup>a</sup> Queenstown is merely mentioned by name, in a list of “Climatic Health Resorts,” on page 7.

a geographical, index. The latter refers to a large map accompanying the work, which was printed at Frankfort-on-the-Main, and for which the author expresses his acknowledgements to Dr. Steinschneider, of Franzenbad, in Bohemia.

One of the opening sentences in Dr. Charteris' handy volume is worth quoting, as it offers an apt explanation of the popularity at all times enjoyed by health resorts:—"An apothecary's mixture," he writes, "is a work of art, often nasty, frequently unseemly. It is nature's handiwork which is carried to the lips, pure and sparkling from the fountains of the well."

At page 37, we come upon the following graphic description of the mud-baths at Franzenbad:—

"The chief attraction, however, at Franzenbad is the excellent mud-baths it possesses. There are mud-baths also at Marienbad and Carlsbad, but these are not to be compared with those of Franzenbad. The reason for this is obvious, for Franzenbad stands on and is surrounded by the materials of which these baths are composed. It is situated on what is known in Scotland as a 'peat moss.' As you tread the turf in the vicinity of the town the shaking as of a yielding elastic floor is felt. When the turf is removed the cause of this is easily explained, for beneath the turf is a stratum, in many cases twenty feet thick, of soft muddy substance, jet black in appearance and moist to the touch. Through it there extend thin plates of iron pyrites, having deposits on the under-surface of fossil plants of the reed and broom. These plates are hard. They are found at all depths, and, after exposure to the air for a few days, become covered with a yellowish-green saline coating, acid to the taste. This bog earth, when touched by the tongue, imparts a sharp acidulous taste of sulphuric acid. Litmus paper is at once reddened when in contact with it, and the smell is like that of sulphuric acid. This muddy earth is brought into the courtyard of the large bathing establishment. It is diluted with water from one of the springs, and then steam is passed through it until, by proper mixing and stirring, it feels like a soft poultice. All undissolved clots are carefully removed. When it is considered sufficiently prepared, a bathing tub resting on four wheels is nearly filled, and moved into the establishment, when it is required for bathing. The temperature at first is high, but it is cooled down until it reaches 80° Fahrenheit. Into this black and unsightly conglomeration the bather, undressed, betakes himself, and rests in it for about twenty minutes. A peculiar fresh, not disagreeable, acid smell is detected, and the effect on the skin is soothing and delightful in the extreme. There is, at first, some slight increase of the respiration and a kind of panting sensation, which, however, soon wears off and is succeeded by a dreamy

quietness. Stepping out of the bath at the expiry of the time mentioned, the appearance presented by the bather is not prepossessing. Here and there the white skin shines through its dark dripping covering, but the general aspect is dirty and repulsive, and the plunge into the fresh clear water of a corresponding bath is relished with glee. Here gradually the peaty covering is removed, and the skin becomes fresh and glowing and satiny to the touch. A kind of feeling as if you are as clean as water can make you remains with the bather during the rest of the day, and the enjoyment is heightened by a sensation as if you had been gently ironed and smoothed like a well-laundried table-napkin. This mud-bath has an excellent effect in cases of nervous excitement and hyperæsthesia. It is a producer of sleep, and calms restlessness, and dispels fidgety sensations."

2. Inspector-General MacPherson's *brochure* conveys the impression that it underwent a rapid enlargement while it was being written—like the schoolboy's snowball, *crevit eundo*. The process is, indeed, clearly set forth in the preface. At first there were notes referring to one group of mineral waters—the lime-sulphated earthy waters, in which sulphate of calcium is the most prominent constituent—*e.g.*, Contrexéville, Bath, Bagnères de Bigorre. Then a few words were added about the carbonate of calcium waters, such as Wildungen and some table waters. Next, a few analytical tables of sodic carbonated waters were included, and lastly, the author was led to make suggestions about the use of the Bath and Bristol waters. Notwithstanding the subject-matter, we have come reluctantly to the conclusion that this is rather a "dry" little book.

3. The third and last booklet on our list may be dismissed with a few words of commendation. In the first place, because the former edition was favourably reviewed in our pages (Vol. LXXVIII. No. 153, page 250, September, 1884), and secondly, because so recently as February last an interesting communication by the author, on "Leamington as a Winter Health Resort," was published in this journal (pages 128, *et seq.*). The present edition includes independent analyses of the constituents of the several springs by Professor J. S. Brazier, F.I.C., F.C.S., Professor of Chemistry in the University of Aberdeen; Dr. Meymott Tidy, F.I.C., F.C.S., of the London Hospital; and Dr. H. Wilson Hake, F.I.C., F.C.S., conjointly with Mr. C. Napier Hake, F.I.C., F.C.S., of Westminster Hospital.

*A Guide to the Examination of the Nose.* By E. CRESSWELL BABER, M.B. Lond.; Surgeon to the Brighton and Sussex Throat and Ear Dispensary. London: H. K. Lewis. 1886. Pp. 163.

WE have read this book with sincere pleasure. It deals only with a limited portion of the subject of nose affections—namely, that expressed by its title, “the Examination of the Nose.” The subject of treatment is only casually referred to in its pages; but a chapter is devoted to the diagnosis of the commoner diseases of the nasal cavities (Chap. VII.). It would, perhaps, have been better, with a view of making the book more generally useful, to have extended the scope of the work, and to have made it a complete treatise on nose diseases. Judging by the merits of the book before us, we could have wished that the author had either taken this view of it, or that he had made this work the first volume, to be followed by a second dealing with the remainder of the subject. Limited, however, as is its scope, it is complete in itself, and treats of the anatomy, physiology, and methods of examination of the nose in a full and exhaustive manner. It is written in a clear style, and ample justice is done to the investigations and opinions of others—French, German, and English alike. This book, unlike many works on special subjects, is remarkably free from those egotistical extravagances which too often disfigure the present-day writings. The first chapter deals with the anatomy and physiology of the nose, and is worthy of close study. The anatomical section is very clear and accurate, and special prominence is given to those parts which are of more practical interest. The naso-pharynx is particularly well described, and this is of the greater importance because it is a region of immense practical importance to the general surgeon as well as to the specialist, and yet it is a region of which, too often, but very hazy ideas exist. The physiology of the nose is dealt with under the heads of Respiration and Smell, Taste, and the effect of the Nasal Cavities on the Voice. In Chapter II., which enumerates the symptoms of nasal disease, an interesting, though rather curtailed, description is given of the reflex phenomena, which lately have assumed a prominent place in current literature. We think, for instance, that the author might have entered more into detail on the subject of cough and asthma in their relation to nasal affections. This would have been more useful to the reader

than to have occupied half a page of footnotes with references as to where information was to be found on the subject. Chapter IV., consisting of 50 pages, gives an admirable account of anterior rhinoscopy, and the various points to be observed in this method of examination. He calls special attention to the assistance afforded by cocaine, a solution of which (4 per cent.), he recommends, should be painted over the inferior turbinated bone. This causes the erectile tissue covering the bone to subside, and thus admits of the examination of the deeper parts of the nose. It also assists in diagnosing hypertrophy from simple erection, and makes the parts more tolerant to examine or treat. Thirty pages are devoted to posterior rhinoscopy, and practical and minute directions are given as to the method of procedure.

In the last chapter the author gives an outline for examining a case, which is minute and accurate, but which it would be very difficult to follow, when a large number of cases had to be examined.

We have no hesitation in recommending this book. It will prove a most reliable guide to those who wish to learn how to examine the nose properly. It is marked throughout with the evidences, not only of extensive reading and theoretical knowledge, but of much practical experience, and all of these the author has succeeded in putting before the reader in an intelligible and interesting manner. It is printed in good type, on good paper, and is illustrated with numerous well-executed woodcuts.

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*Sore Throat.* By PROSSER JAMES, M.D.; Lecturer on Materia Medica and Therapeutics at the London Hospital, &c. Fifth Edition. London: J. & A. Churchill. 1886. Pp. 363.

WE had occasion to review the fourth edition of this book, so that the present volume requires but short notice. We then commented strongly on what we considered its cardinal defects, and we regret to say that the present edition perpetuates and emphasises them. We cannot look upon this book as a reliable guide to the "nature, varieties, and treatment" of sore throat. It is shallow, and written in an unpleasantly egotistical style. In the preface the author apologises for the delay in its issue. The apology consists in a page of advertisements of the other works published by Dr. James. First, a volume on "The Therapeutics of the Respiratory Organs" for "Woods' Library of Standard Medical



Authors." "That work, considerably larger than this, appeared in New York last December and the following April in London." Then an enlarged edition of my "Laryngoscopy and Rhinoscopy," and, thirdly, a "Guide" to the British Pharmacopœia. "A second edition of that 'Guide' was called for almost immediately, and caused still further delay."

We find two hand-coloured plates in this book, which are not to be compared with those we have been accustomed to see in other works on the throat. The first of these represents the pharynx studded over with little green and blue spots, and on referring to page 169 we are informed that it is a faithful representation of granular or follicular sore throat. The picture is, if possible, more defective than the description the author gives. These prominent follicles are "red and angry looking, or paler, or yellowish, according to the contents of the enlarged follicles, which may be mucous or purulent." These yellowish spots are, "in fact, little pustules formed by suppuration of the inflamed follicles." It seems extraordinary that an author writing on sore throat, and, therefore, presumably acquainted with his subject, should fail to recognise the fact that diseased follicles rarely suppurate, but that the yellow appearance they so frequently present is due to caseation of their contents. His treatment, too, of this affection is, we think, not likely to meet with much success in the hands of others. The therapeutic measures we may pass by without comment, but we cannot endorse Dr. James' local treatment. Perchloride of iron as a spray, painting with iodine, iodoform in a powder, or painting the surface with nitrate of silver solution—2 grs. to 60, and sometimes 120 grs. to the oz.—may afford temporary relief, but will not cure enlarged follicles. The best local remedy, and the least painful to patients, the galvano-cautery, is dismissed with the remark that—"I have not found that destruction of tissue is desirable."

In the present edition the author appends a chapter on external sore throat, apparently with the object of giving a summary of Mr. Horsley's Brown-Lectures for 1884 and 1885, dealing with Thyroidectomy, from notes taken at the lectures by the author, as the lecture had, "in fact, not yet been printed." In this chapter we find a valuable contribution to the literature of Exophthalmic Goitre, which we shall quote in the author's own words:—"In *Exophthalmic Goitre*, or Graves' disease—or, as the Germans call it, Basedow's disease—we have besides the enlarged thyroid, the

conditions of the eye and the heart to consider. The rapid, general tremors are mentioned in the last paragraph. They are most interesting cases, though their management is attended with difficulty. Hence they are too often neglected." We presume the author has found no original method of treatment for these difficult cases, or we should have had a few extra pages devoted to the subject.

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*An Introduction to the Use of the Laryngoscope.* By ARCHIBALD E. GARROD, M.A., M.B., Oxon. London: Longmans, Green, & Co. 1886. Pp. 54.

THIS book is nothing more than what it pretends to be—"a handy guide to practitioners who may wish to acquaint themselves with the use of the laryngoscope, and to the daily increasing number of students who attend in the throat departments of our hospitals."

In the first chapter a description is given of the laryngoscope, which, though very short, is clear, and gives the essential points to be observed in choosing an instrument. There is one point on which we cannot agree with the author. He states (p. 5)—"Whether the reflector be worn in front of one eye, or over the forehead, is a matter of individual taste. Neither place has any particular advantage." A knowledge of the elementary laws of the reflection of light ought to satisfy Dr. Garrod that to be able to see to the best advantage the illuminated field in the larynx the eye should be in the centre of the rays reflected from the frontal mirror. In Chapter II. the manner of using the laryngoscope is explained, and in Chapter III. the difficulties likely to be met with by the beginner. The chief of these are two—intolerance on the part of the patient, and "dependence" of the epiglottis. For beginners, wishing to practise the art, a point alluded to by the author will suggest the best class of cases to examine:—"In some, especially in hysterical people, the parts are so little sensitive that reflex action is not induced even by the most clumsy movements of the throat mirror against the back of the pharynx." For difficulty arising from "dependence" the author does not mention a plan which will often succeed when all other plans, short of "lifting" the epiglottis by Türck's or Schrötter's method, have failed—that is, by making the patient laugh while the mirror is in position. The whole larynx will then become visible, though previously no part of its interior could have been seen. The

laryngoscopic picture, the anatomy and physiology of the larynx, the application of remedies, and laryngeal surgery, are then very shortly discussed. Six pages are then devoted to anterior and posterior rhinoscopy; we think the author has done well in doing so, as in the majority of cases a knowledge of the condition of the naso-pharynx, at least, is essential in the diagnosis of throat affections. We cannot endorse the author's direction (p. 45) as to the introduction of the mirror. It should not be brought to rest against the back of the pharynx. Posterior rhinoscopy requires extreme delicacy and care so as to avoid reflex irritation even in tolerant throats. By bringing the mirror to rest close to the posterior wall of the pharynx without touching it, the image can be quietly studied, but in most cases the moment the pharyngeal wall is touched, reflex action is excited and the mirror must be withdrawn.

On the whole we can recommend this book to beginners. It is clear, concise, and not overburdened with detail, and will give a fair idea of the use of the laryngoscope to those unacquainted with it. Those who have seen anything of throat work—who have had even a month's experience at a throat hospital or a throat department of a general hospital—will have learned quite as much as this guide can teach them, and a larger work on the subject will be more useful to them.

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*Medical Boycotting.* By DR. R. E. DUDGEON. Reprinted from the *Homœopathic Review*.

DR. DUDGEON is very angry, and his wrath has boiled over the edge of the *Homœopathic Review* into a pamphlet. *The Lancet* declined to print an unconscionably long letter of his, seeing "no good to be done by any lengthened discussion of the principles of Hahnemann;" the publishers of *The Practitioner* refused to insert an advertisement of Dr. Ameke's "History of Homœopathy" (recently reviewed in our pages); and a specialist refused to meet Dr. Dudgeon in consultation. We own to some sympathy with him in his second grievance; medicine will not perish from the earth when the history of homœopathy is fully known. We do not deprecate discussion of our principles or of Hahnemann's, and we are willing to learn from Hahnemann or Dr. Dudgeon any truth he has to teach. The latter gentleman will not teach much or many by such letters as these which he proudly prints and reprints.

Their acrid tone and imputation of paltry motives will not win converts even to truth; and his horrible vindictiveness in withdrawing his subscription from *The Practitioner* can only excite sympathy for the ruined—even though erring—publishers.

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*Surgical Handicraft*. By WALTER PYE, F.R.C.S.; Surgeon to St. Mary's Hospital and to the Victoria Hospital for Sick Children, &c. Second Edition. London: Henry Kimpton, 1885. Pp. 546.

THIS book professes to be "A Manual of Surgical Manipulations, Minor Surgery, and other matters connected with the work of House Surgeons and Surgical Dressers," and that it has been appreciated as such is proved by the rapidity with which it has run into a second edition. It does not pretend to treat of those larger surgical measures which are met with in the text-books of surgery—such as amputations, excisions, or the ligature of arteries—but in the field which Mr. Pye has chosen to cover, he has done his work well, and has produced a book which not only house surgeons and dressers will find useful, but from which even hospital surgeons may derive suggestions and information. This volume is not so large as the former one, owing to the "excision of unnecessary verbiage." It is also better printed, and the price has been reduced. It is illustrated with 233 woodcuts, for the most part clear, accurate, and excellently executed. The chapters on bandaging and the arrest of hæmorrhage are perhaps the best in the book, though all are good. The same may be said for Appendix No. II., which treats of the preparation of patients for operation. The only chapter which we find exception to is Chapter XVI., which deals with antiseptic dressings. The tone here would suggest that Mr. Pye is not a very fervent believer in the method, or has not been very successful with it. The note with which he prefaces the chapter which is practically the same that appeared in the first edition, is full of doubt and hesitation. Thus, one paragraph runs—"While the value of antiseptic dressings in preventing septic absorption are as much allowed as ever, the opinion is gaining ground that these results may be obtained in more simple ways than heretofore, and faith is again being more placed in the capacity of healthy tissues in a wound to heal aseptically and to be unaffected by germ elements in their neighbourhood, provided that they are so treated as to be kept in

health throughout." A certain uncertainty as to the strength of the antiseptic lotions seems also to prevail. Thus (page 213) a lotion of carbolic acid for cleansing an incised wound we are told ought to be 1 in 40 to 1 in 100; at page 522, all instruments to be used at an operation "are generally placed in trays containing carbolic acid solution, 1 part in 60 to 1 in 120." At page 232, the instruments are to be placed in a 1 in 40 solution, the sponges in 1 in 20, the operator's hands are to be thoroughly rinsed in a 1 in 40 solution. In opposition to p. 213, already quoted, we are here informed that "the edges and neighbourhood of an *accidental* wound, or the skin surface where an operation wound is about to be made, must be washed, or rather scrubbed, with the 1 in 20 or 1 in 30 carbolic solution." These discrepancies are rather bewildering, but we believe it is the general opinion of experts in the antiseptic method that the weaker solutions, 1 in 60 or 1 in 120, are not sufficiently effective. It is also a matter of regret that more prominence has not been given to the later improvements in the method which render it more easily, and therefore more generally employed.

With these minor defects our criticism ends. The book is one of considerable merit, and is certain to prove of great and widespread utility.

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*Handbook of the Diseases of the Nervous System.* By JAMES ROSS, M.D., LL.D. London: J. & A. Churchill. 1885. 8vo. Pp. 723.

THE "Treatise on the Diseases of the Nervous System" by Dr. Ross has more than once been favourably noticed in these pages, and, as a work of reference on the subject with which it deals, it has few equals. It is characterised by great comprehensiveness, and enters largely into details, and, treating of such a vast subject, is necessarily a very large and expensive book. To meet the wants of students and of such practitioners as have not much time for reading, the author has prepared the smaller work before us.

This is divided into two parts. In the first we find chapters on the Anatomy and Physiology of the Nervous System, on General Morbid Anatomy and Physiology, General Symptomatology and General Treatment. In this part the author develops his favourite theories of evolution and dissolution of nervous structures and functions. The second part deals with the Special Pathology of the Nervous System, and here the aim has been successfully followed

to make the work as thoroughly practical as possible. A clinical classification has been adopted, "so that the diseases which are most apt to be mistaken for each other will be found described in close proximity, and the reader can thus note the various features which differentiate nervous diseases clinically allied." In general, the clinical descriptions are full, and space is gained by omission of details of morbid anatomy and physiology and by leaving out references to the opinions and writings of authorities. In this way, in a comparatively short compass, we have a practical treatise on diseases of the nervous system, for which it would be hard to find an equal at the present time. It is a work which we can unreservedly recommend to our readers, and we feel sure that it will add largely to the well-earned and firmly-established reputation of its author.

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*Outlines of Infectious Diseases, for the Use of Clinical Students.*

By JAMES W. ALLAN, M.B., Physician-Superintendent, City of Glasgow Fever Hospital, Belvidere. London: J. & A. Churchill. 1886. 8vo. Pp. 120.

IN July, 1880, we noticed, with approval, in the pages of this journal (Vol. LXX., page 45), a little book by Dr. Allan, of the Belvidere Fever Hospital, Glasgow, entitled "Notes on Fever Nursing." It gives us much pleasure on the present occasion to express an equally favourable opinion as to the merits of a more recent literary venture by the same author, which has been made in the interest of all clinical students of fever, but primarily of those attending Dr. Allan's own *clinique* at Belvidere.

In the "Outlines of Infectious Diseases" will be found a generally accurate, clear, and concise epitome of the phenomena presented by the eruptive and continued fevers of this country, their ætiology, diagnosis, and treatment. Of necessity, the literary merit of the book is more or less sacrificed on the altar of Brevity, but bad grammar is conspicuously absent. Every page bears evidence of careful clinical observation, and the word-pictures of disease are true to the life—drawn, beyond all doubt, by a man who has his models before his mind's eye. We may mention that chapters or sections on whooping-cough, diphtheria, and cholera, are included in the contents.

There are, of course, some points to which exception may be taken. For example, we do not think it necessary—nor is it often possible in fever cases—to leave the clinical thermometer "in the

axilla ten minutes or a quarter of an hour," as recommended on pages 4 and 29. Again, Dr. Allan says (on page 10) that "second attacks of typhus" are "practically unknown." It is curious that he should make this rather dogmatic assertion immediately after quoting Murchison's name. If we mistake not, that classical writer on fever himself suffered three attacks of typhus—certainly two. Nor has an experience of many years at Cork-street Fever Hospital led us to become enamoured with chloral as a hypnotic in typhus, *malgré* the "great advantages" claimed for it as such by Dr. J. B. Russell, of Glasgow, and by Dr. Allan. It has appeared to us to be an uncertain remedy, and one not free from risk. Given in small doses, it is apt to excite the patient, as pointed out by the author; in large doses (20 to 30 grains), it *may* unfavourably influence the already weakened heart. In connection with the treatment of the wakeful delirium of typhus, Dr. Allan observes (page 16), that "Grave's (*sic*) mixture may be given, also with care and caution." By-the-by, Dr. Allan does not mention the composition of Graves' mixture. We think that the combination of digitalis and opium with spirit of nitrous ether, recommended by Murchison, is preferable either to chloral with morphine, or to Graves' tartar emetic and opium mixture.

But a few debatable points such as the foregoing, will not seriously detract from the value of what is really an excellent epitome, written with an enthusiasm which is transparent on every page, and based on a practical acquaintance with the "ways of the sick," as they may be observed in the wards of a fever hospital.

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*Clinical Lectures on the Practice of Medicine.* By the late ROBERT J. GRAVES, M.D., F.R.S. London: The New Sydenham Society. 1884.

GRAVES' Clinical Lectures need no eulogium now, and, although more than forty years have passed since the first edition of them was published, they still maintain a worthy place among medical classics, and will amply repay a careful perusal.

The first edition appeared in 1843, and the second in 1848, under the editorship of the late Dr. Neligan. This second edition was reprinted in 1864, prefaced by an introduction by the late Professor Trousseau for the French edition of the book; and the volumes under notice are an exact reproduction of the second edition.

Trousseau characterised Graves as a perfect clinical teacher, "an attentive observer, a profound philosopher, an ingenious artist, an able therapist; he commends to our admiration the art whose domain he enlarges, and the practice of which he renders more useful and more fertile."

### RECENT PERIODICALS.

1. *The Transactions of the Medico-Chirurgical Society of Edinburgh*. Vol. IV. New Series. Session 1884-85. Edinburgh: Oliver & Boyd. 1885. Pp. 286.
2. *Transactions of the American Surgical Association*. Vol. III. Edited by J. EWING-MEARS, M.D., Recorder of the Association. Philadelphia: P. Blakiston, Son & Co. 1885. Pp. 396.
3. *Transactions of the Medical and Chirurgical Faculty of the State of Maryland, at its Eighty-seventh Annual Session, held at Baltimore, Maryland, May, 1885*. Baltimore: Thomas & Evans. 1885. Pp. 253.
4. *Southern California Practitioner*. Vol. I. No. 3, March, 1886. Los Angeles, California. Pp. 40.
5. *The Polyclinic*. A Monthly Journal of Medicine and Surgery: conducted by the Faculty of the Philadelphia Polyclinic and College for Graduates in Medicine. Vol. III. No. 7. Philadelphia: 1886. Pp. 16.
6. *La Cronica Medica, Organo de la Sociedad "Union Fernandina."* No. 21. Setiembre de 1885. Lima: Carlos Prince. Pp. 46.
7. *Revista Argentina de Ciencias Médicas*. Número 12. Diciembre de 1885. Buenos Aires: Stiller & Laass. Pp. 30.
8. *La Rassegna di Scienze Mediche, Organo ufficiale della Società Medico-Chirurgica di Modena*. No. 2. Febbraio, 1886. Modena: G. T. Vincenzi e Nipoti. Pp. 48.

1. THE Transactions of the sixty-fourth session of the Medico-chirurgical Society of Edinburgh include twenty-two original papers, all interesting and some of special value; 27 patients, 48 pathological specimens, and 30 "miscellaneous objects," were exhibited at the sittings. The discussions on the papers are reported at considerable length and add largely to the interest and value of the published Transactions. We shall mention only four of



the "original communications," although none will be read without advantage. Dr. Littlejohn (President of the Society), Lecturer on Medical Jurisprudence, reports a case of homicidal poisoning with nitrate of potassium, which he believes (and we believe) to be "the only case of criminal administration of nitre hitherto recorded." How the poison was administered to a child of three years, and how much was given, could not be ascertained. The clue was supplied to the analyst by the discovery of a few minute crystals on the toe of one of the child's boots. The author makes the case the occasion for giving some useful "hints as to the conduct of medical practitioners in cases of suspected poisoning." Prof. T. Grainger-Stewart reports a successful case of tapping the pericardium. He believes that the patient would have died but for the operation. He gives the recorded statistics which show 97 cases, of which 38 (39 per cent.) were successful. He concludes that the operation, though rarely required, is in certain cases justifiable. Dr. Clouston's paper, on "The Position of the Medical Profession in regard to Certificates of Mental Unsoundness and Civil Incapacity," enters very fully into the history of the relation between the medical profession and the law in the matter of certificates and evidence of insanity. The subject is of great importance at the present time, and both the essay and the subsequent discussion are valuable contributions towards the settlement of some vexed questions. "The Etiology of Rickets" is the subject of an elaborate paper by Mr. J. Macdonald-Brown. He rejects, and apparently on good grounds, the diathetic theory. The "hygienic theory"—that "the disease is due to any cause which impairs or lessens the general well-being of the child during the early months of life, such as bad air or surroundings, want of light, improper feeding, and all the other ills of poverty—he considers inconsistent with the facts. There are obviously immense difficulties in the way of bringing so complex a question as the ætiology of rickets to the test of statistics; but Mr. Brown has made the attempt, with the following results:—He selected (in the course of four years) 100 rickety children under three years of age, and inquired carefully into the dietetic history, the surroundings, and the family history of each. He found that 71 had been fed on milk, practically alone, for more than nine months; that 20 had had milk "largely supplemented with arrowroot, breadsop, &c.;" and that in 9 cases only had over-lactation been carried to any extent. He found that 68 of the children lived in fairly comfortable homes, well-ventilated

and lighted; 21 in "circumstances which could not be described as favourable;" 11 in squalor and dirt. As to family history—he found a scrofulous taint in about 20, a syphilitic in about as many, maternal delicacy before or during pregnancy in 30. No satisfactory evidence of hereditary transmission of the disease was discovered. On the other hand, of the 100 *non-rickety* children 60 were fairly healthy, 40 weakly. Of the latter, 30 had been fed in early infancy on starches with or without milk; the sanitary surroundings of more than half were unfavourable; more than a fourth were strumous; six were tainted with syphilis; and in most of the forty cases there was evidence, more or less trustworthy, of infirm health during or before pregnancy. Of the 60 healthy children 30 had been brought up on "milk with farinaceous matter, the milk in some instances being almost dispensed with." In 22 cases the sanitary conditions were unfavourable. We give Mr. Brown's conclusions—in favour of the "dietetic theory"—in his own words, and we commend the entire essay to the study of our readers:—

"The fact that a large proportion of the rickety children had (during the early months of life) milk diet, appears to me to militate strongly against the dietetic theory of the etiology of rachitis—while the hygienic hypothesis seems to be largely negatived by the fact that in nearly 70 per cent. of the same class of cases the hygiene and surroundings of the children were good. It must be confessed that the results of these investigations are not altogether satisfactory, inasmuch as while they are negative of the dietetic and hygienic theories, they do not furnish a more positive hypothesis to replace them. Judging from these statistics it, however, seems possible to me that a rickety constitution may exist, the effects of which can only be manifested during the earliest years of child-life. If such exist, it is exceedingly wide-spread, and is not specially limited to any country, or to any particular district of a country." (P. 211.)

2. American medicine and surgery are well known and fully appreciated by the profession at this side of the Atlantic, through the great works of Gross and Hamilton, Emmett, Thomas, Sims, and others, whose names are familiar to us all; but the activity and success with which every department of our science and art is studied in the western continent is even more evident in the periodical medical literature than in the standard works. Every city or town of any considerable importance maintains its weekly or its monthly journal devoted to professional subjects. Medical societies great and small abound, and many of them periodically

publish their proceedings. There is scarcely any of these publications from which something useful may not be gleaned, while some of them are not surpassed in interest and value by any European production of corresponding character. To this latter class belongs the third volume, now before us, of the *Transactions of the American Surgical Association*, which records the proceedings of the session held at Washington in April of last year. The Association was founded in 1880 by Dr. Gross, whose death, on the 6th May, 1884, at the age of seventy-nine, was mourned by surgeons throughout the world. In the fifth year of its vigorous childhood the Association numbered nearly one hundred Fellows; and we gather from some remarks of the retiring President that the roll might be indefinitely lengthened if certain restrictions, depending on a feeling of exclusiveness entertained by the original Fellows, were removed. Of fourteen Honorary Fellows, Mr. Annandale represents Scotland: Erichsen, Sir Joseph Lister, and Sir James Paget, England. Of the eleven papers read during the 1885 session, the first (and, with one exception, the longest in the volume) is an exhaustive treatise on "The Field and Limitation of the Operative Surgery of the Human Brain," by Dr. J. B. Roberts, of Philadelphia. The essay occupies 69 octavo pages, and the discussion upon it 39. The nine propositions which he seeks to establish are—that the symptoms interpreted generally as arising from "compression of the brain," are due to some kind of intracranial inflammation; that conversion of a simple into a compound fracture by incision of the scalp is attended with little increased risk; that removal of portions of the skull is not more dangerous than "amputation of a finger through the metacarpal bone;" that in most cranial fractures the inner table is more damaged than the outer; that "perforation of the cranium is to be adopted as an exploratory measure almost as often as it is demanded for therapeutic reasons;" that drainage is more necessary in wounds of the brain than in others; that "many regions of the cerebral hemispheres in man may be incised and excised with comparative impunity;" that injuries to membranes, arteries, or sinuses, should be treated like similar lesions elsewhere; and that "the results of the study of cerebral localisation are more necessary to the conscientious surgeon than to the neurologist." Each of these propositions is elaborately considered in detail, and, in connection with the last, maps and rules are given for marking on the shaven head the position of the cerebral convolutions which are important to the surgeon. The principles thus laid down are

applied to treatment of cranial lesions and their consequences. The discussion which followed the paper, and which is reported in full, is of great value. We cannot forbear quoting from it the case of a negro man in the Central Lunatic Asylum of Richmond :—

“We had no history of the case, and none could be obtained. He would sit for hours crouching by the wall of his room, and no one in the hospital had ever heard him speak. He had frequent epileptic fits and occasional attacks of violent mania. I trephined his skull and removed the depressed bone—a part of the left parietal bone. A few hours later I went into the room and said ‘Good morning.’ He replied, ‘Good morning; where is the army to-day?’ That was the first intelligent word he had uttered. I said, ‘Where was the army yesterday?’ ‘It was at Manassas,’ he replied, referring to a period about nine years previous. Between this period and the time of the operation, nine years, his mind was a perfect blank. He told me afterwards that he had been struck with a boat-hook by a companion. He remembered the man approaching him with the hook, and then lost all recollection until the removal of the bone. This man recovered, and worked as a common labourer for years after.” (P. 71.)

In the second paper, on “Nephrectomy,” Dr. Samuel W. Gross, of Philadelphia, gives summarised details of 233 cases of extirpation of the kidney, all that he had been able to collect up to date. The mortality was 44·65 per cent. The incision was lumbar in 111 instances, with a mortality of 36·93 per cent.; ventral in 120, with a death-rate of 50·83. So far as these figures go, the fatality of the abdominal operation exceeds that of the lumbar by 13·90 per cent. The longest essay in the volume (extending to 106 pages) is “An Experimental and Clinical Study of Air-Embolism,” by Dr. Senn, of Milwaukee, in which everything that can be said on an interesting, though not very practical, subject, is said. Dr. Mears, of Philadelphia, contributes an excellent paper “On Phosphorus Necrosis of the Jaws.” His researches lead him to the conclusion that the primary effect of the intoxication is the production of a morbid state of the periosteum, which develops into inflammation and necrosis, under the influence of an exciting cause, such as a carious tooth or accumulation of tartar. For prevention he relies upon ventilation and the vapour of turpentine, with attention to the condition of the teeth and gums.

3. The Medical and Chirurgical Faculty of the State of Maryland held its 87th annual session at Baltimore, in May, 1885. Its Transactions include Addresses, Reports of Committees

of eight Sections, and eight papers. The President, Dr. Latimer, chose the origin and diffusion of cholera as the subject of his Address, but adds little to the information already collected by Hirsch. Antipyrin and cocain receive a good deal of attention in this volume, and the virtues of both are extolled. Of the latter we may note that the hydrochlorate is liable to rapid deterioration, due to fungous development at the expense of the alkaloid; and that the vitiated drug may produce inflammation. Of the "volunteer papers," Dr. Cathell's "On Rectal Medication" is the most important.

4. Let us go farther south and glance at the third number of the *Southern California Practitioner*, a monthly, published at Los Angeles, which gives fifty octavo pages, on good paper and in good type, for sevenpence-halfpenny. It contains original articles, selections, editorials, "specials," and even funny stories—only two and little ones—under the title of "placeboes." "The Methods of Capital Punishment" is the subject of the opening paper by a Professor of Obstetrics. No poison fulfils the essential conditions of instantaneous, painless, bloodless, impressive action; and Dr. Lindley prefers the gallows (with a short drop, because "a man could do a great deal of terrible thinking during the time he would fall the extra two and three-fourths feet recommended by Prof. Houghton") to axe, bullet, garotte, or electric current. There is an interesting account of "Leprosy on the Hawaiian Islands." Periodical leper-hunts are organised, and the captures sent to a separate island (Molokai), where 2,000 of these unhappy people are now segregated. The natives trace the disease to China; and the first case amongst them is said, on good authority, to have occurred in 1838, in a family in which a leprous Chinese servant was employed. Of the contagiousness of the disease, as of its incurability, no doubt is entertained.

5. *The Polyclinic*, a monthly journal published in Philadelphia, is in its third year. In character it corresponds to our *Practitioner*. The matter is all up to the average, and none of the papers demand special notice.

6. *La Cronica Medica*, a Spanish journal published monthly in Lima, has not yet completed the second year of its existence. Much of its October number, now before us, is occupied with a furuncular disease called "verruca," endemic in certain districts of

Peru. A young physician, S. Daniel Carrion, died last August, thirty-eight days after having had himself inoculated, for experimental purposes, with matter from one of the tumours. A special bacillus is supposed to be the immediate cause of the disease, which is described as zymotic and contagious, accompanied by a febrile condition. No satisfactory explanation of its occurrence only in certain regions can be offered. With respect to the too successful inoculation, it is worth mentioning that the police authorities ordered an investigation, holding that S. Carrion's death might be considered a suicide, or that the physician who performed the inoculation (seeing that the young man was determined, otherwise, to do it himself) might be guilty of "qualified homicide." One of the official documents which the inquiry produced is an elaborate defence of patient and operator by Dr. Leonardo Villar. He enumerates European instances of similar experimental inoculations, one of which is as follows:—"In the last cholera epidemic in Egypt, the celebrated Dr. Koch administered to Dr. Strauss an intra-venous injection of the blood of a cholera patient. This injection caused the death of Dr. Strauss, but the operation was not considered homicidal." The remainder of the number is mostly occupied by a lecture on carbuncular affections, and intra-venous injections of ammonia as a specific for them, addressed to the members of the "Union Fernandina" (of which *La Cronica Medica* is the organ), by S. Leonidas Avendaño\*; by the continuation of a paper on the mineral waters of Peru; and by some statistics, chiefly obstetrical.

7. *The Revista Argentina de Ciencias Médicas*, a Spanish monthly, published at Buenos Ayres, has completed its second annual volume. The December number opens with an argument against a recent rule of Life Assurance Companies to impose a surcharge of one per cent. on officials of hospitals for contagious diseases. Dr. José Penna maintains that a process of acclimatisation to noxious atmospheres takes place, rendering medical attendants and others infection-proof. There are two other

\* S. Avendaño concludes his lecture with these words:—"If my anticipations are realised, and other cases in which ammonia is employed have the same fortunate issue as those which I have cited, to Peruvian medicine will belong the glory of having initiated and established four instances of treatment by intravenous injections: chloral for tetanus (Dr. Lino Alarco); ammonia for purulent infection (Dr. Lino Alarco); capsicum for yellow fever (Drs. Armando Vélez and F. P. del Barco); and ammonia for carbuncle (Drs. Néstor Corpancho and L. Avendaño).

original papers, one on hydrarthrosis and one on dilatation of the stomach in children.

8. We shall now return to Europe, and conclude with mention of a monthly journal, *La Rassegna di Scienze Mediche*, the organ of the Medico-Chirurgical Society of Modena. It is but an infant, in its second month in February last. The second number contains three original papers—two surgical and one medical—the proceedings of the Society, and much miscellaneous information on professional subjects.

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*Von Ziemssen's Handbook of General Therapeutics.* Vol. III. *Respiratory Therapeutics*, by PROFESSOR M. J. OERTEL, M.D. Translated, with a Preface and Notes, by J. BURNEY YEO, M.D., F.R.C.P.; Professor of Clinical Therapeutics in King's College, London; Physician to King's College Hospital, &c. With 88 Illustrations. London: Smith, Elder & Co. 1885.

UNDER the somewhat unfortunate title of "Respiratory Therapeutics," the third volume of the translation of von Ziemssen's "Handbook of Therapeutics" treats of a subject to which but comparatively slight attention has been paid in this kingdom. A work of this kind, emanating as it does from so important an authority on pulmonary diseases as Professor Oertel, and translated by so well known a specialist, as we may call him, as Dr. Burney Yeo, will probably lead many to make themselves better acquainted with the methods recommended, and to put them to an actual test.

There is no doubt that the inhalation treatment has not had a satisfactory trial with us. Many who have employed it have done so empirically, or without any accurate knowledge of the subject, and, accordingly, the result has often been unsatisfactory. One reason of this may possibly have been the want of any English work on the subject; for, until the publication, about this time last year, of Dr. Arthur Hill Hassall's excellent work,\* there was no systematic or practical information available to the English reader, except, indeed, the work of Dr. Cohen, of Philadelphia, the last edition of which appeared, we believe, ten years ago. Theoretically it would seem more logical that affections of the respiratory system should be treated by measures acting directly

\* *The Inhalation Treatment of Diseases of the Organs of Respiration, including Consumption.* By Arthur Hill Hassall, M.D. 1885.

on or through that system, than through the digestive system or some other roundabout channel. Professor Oertel shows that this practically can be effected in a large number of cases. He describes in minute detail, and gives illustrations of, the apparatus used for the inhalation of fluids, vapours, gases, and solid substances; also the modes of practising inhalation, the remedies adapted for inhalation, classified according to their action; and the treatment of special maladies by inhalation.

This, which is called the "Chemical Part of Respiratory Therapeutics," comprises the first half of the volume. The remainder is made up of the "Physical Part of Respiratory Therapeutics," or, as it is termed, "Pneumatic Therapeutics," and deals with the treatment of diseases of the respiratory and circulatory system by alterations of air pressure, acting on the pulmonary surface, and acting generally as in the pneumatic chamber.

These two methods of treatment—the physical and the chemical—as the author shows, for example, in the case of emphysema, mutually and necessarily supplement each other. As yet the employment of these methods of treatment is in its infancy; but the fact of their now being based on scientific grounds must soon secure for them a wide field of employment.

It would be useless to attempt to give our readers any analysis of a work so largely made up of technical and scientific details and descriptions of special modes of treatment in various maladies. Reference to the volume itself, or to some such work as Hassall's, could alone be satisfactory to anyone honestly desirous of efficiently carrying out the inhalation treatment of diseases of the respiratory organs. Unfortunately, the therapeutic employment of compressed air is at the present time greatly restricted, since pneumatic chambers exist but in a very few places. So far as we know, there is not a single one in Ireland, and but one or two in England. Of all pulmonary affections those which are connected with phthisis, according to Oertel, present the strongest indications for the employment of compressed air. This, he states, is a well-founded opinion which should be more widely spread among physicians as well as patients.

We have noted many important therapeutical points in our perusal of the first part of the work, which, however, for the reasons already stated, we shall not particularise. It is advisable, however, to call attention to the remark that, in such diseases as diphtheria, whooping cough, and gangrene of the lung, in which



inhalation of carbolic acid is used by some practitioners, it is essential that stronger solutions than have hitherto been employed should be ordered. Oertel always employs a 5 per cent. solution, as there is, he says, no certainty of success with weaker ones. He gives the same solution by means of his steam pulveriser to phthysical patients in inhalation, four to six times a day, especially when there is profuse expectoration, partly to destroy the deleterious influence of the putrid and putrefying secretions of the bronchi and cavities upon the respiratory mucous membrane and the lungs, partly to reduce their infective property as much as possible before their eventual suspension in the air, and to protect other persons from infection.

As was the case with the former volumes of this Handbook, the present work is an exhaustive one, and the translation has been ably done by Dr. Yeo.

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*New Sydenham Society Lexicon of Medical Terms.* Parts IX., X., XI. Ect-Gly.

STEP by step this ponderous undertaking wends its way through the alphabet, and with the 11th part commences the 3rd portly volume. We can imagine the joy with which the editors will, at some distant day, welcome the completion of their herculean task, and we heartily wish them continued success in their labours. An immense amount of the most varied information, much of it quaint and curious, is to be found in the pages of this work.

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*Atlas of the Cutaneous Nerve-supply of the Human Body.* By JACOB HEIBERG, M.D. Illustrated by A. FOSTERED. Translated and Edited by W. W. WAGSTAFFE, F.R.C.S. London: Baillière, Tindall, and Cox. 1885.

IN 10 plates the distribution of the cutaneous nerves of the body is illustrated by means of differently coloured areas, and Mr. Wagstaffe was induced to present Professor Heiberg's atlas to English readers by the belief that such a plan has an advantage over the outlined areas which are given in some works.

The diagrams represent the anterior and posterior surfaces of the entire body; the head and face, anterior and posterior view; the arm and leg, front and back; and the hand and foot. The student

is warned against being misled by the fact that the blending of nerves from adjoining areas is purposely omitted so as to render the plates clearer. In the out-patient room, and in the hospital wards, these diagrams will prove useful in facilitating the study of affections of the skin and nervous system, and Mr. Wagstaffe has added some judicious explanatory notes which enhance the usefulness of the atlas.

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*A Manual of Dermatology.* By A. R. ROBINSON, M.B. With 88 Illustrations. New York: Appleton & Co. 1885.

THIS volume purports to be the modest forerunner of a "much larger, more pretentious, and more original work," which the author intends to present us with at some future time.

Dr. Robinson has made himself favourably known by his investigations on the morbid histology of various diseases of the skin; and the majority of the illustrations are original, either woodcuts or reproductions by the photo-engraving process.

Histology, in fact, is the strong point of the book; for, in other respects, the contents are not above the average, and the clinical descriptions of the different affections follow the well-worn tracks.

Auspitz's classification of skin diseases is commended for advanced dermatologists, but Hebra's grouping is preferred as best suited for purposes of diagnosis. In Dr. Robinson's opinion the classification adopted by the American Dermatological Association "was decided by balloting, and never should have seen the light." (!)

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#### TREATMENT OF FACIAL NEURALGIA BY COCAIN.

DR. DE CONINCK, of Ledeberg-les-Gant, writes to the *Scalpel*, of Liège (*Le Moniteur du Practicien*, Jan. 15), that the effects of hydrochlorate of cocain in facial neuralgia, and in cephalagia having its seat in the temporal region, are surprising. The pain, be it ever so intense, will instantaneously cease on applying to the auditory canal one minim of a solution of 1 per cent. of this salt, by means of a small camel-hair brush. This signal effect, however, will only continue for a few hours, after which a repeated application may be required. Hydrochlorate of cocain has never failed in the many cases of these kinds of neuralgia, treated in that manner by Dr. de Coninck. In neuralgia of the fifth nerve and its branches, however, the results were less certain and less satisfactory, owing, perhaps, to the superficial mode of its employment.—*London Med. Record*, March 15.

## PART III.

### HALF-YEARLY REPORTS.

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#### REPORT ON FORENSIC MEDICINE.

By H. C. TWEEDY, M.D., DUBL.; M.K.Q.C.P.; Diplomate in State Medicine, Trin. Coll. DUBL.; Fellow and Examiner Royal College of Surgeons; Physician to Steevens' Hospital.

1. An Unusual Case of Gunshot Wound of the Heart.
2. Schultze's Method of Treatment of Asphyxia of the New-born.
3. Still-born Child Resuscitated Forty-five Minutes after Birth by Artificial Respiration.
4. Feigned and Artificial Eruptions of the Skin.
5. Homicide and Mutilation.
6. Lung Test in Infanticide.
7. Suicide and its Statistics.

#### I. AN UNUSUAL CASE OF GUNSHOT WOUND OF THE HEART.

FROM the *Philadelphia Medical News* we get the following points of a case reported at the *stated meeting*, Jan. 6, 1886, of the Baltimore Academy of Medicine, by Dr. J. Edwin Michael. On Christmas night, about nine o'clock, he was called to see a large, stout, well-built German man, a saloon-keeper, who had received a pistol-wound while endeavouring to quell a disturbance. The ball came from a pistol of the "bulldog" pattern, 38 calibre, shot from a distance of twelve to fourteen feet, in front and a little to the right of patient. The entrance of the ball was quite close to the apex—that is, three inches to the left of the median line, one and a half inches to the right of the left nipple line, and three-quarters of an inch below the horizontal nipple line. The condition of shock was extreme; stimulants were given, and restored consciousness. Eleven hours after the accident the patient complained of severe general pain. This was relieved by morphine with atropine, hypodermatically. Twenty hours from the time of the accident the patient died.

The autopsy showed that the ball had taken an oblique direction through the fifth rib, near the cartilaginous junction, through the pericardium, through the left ventricle, ploughing a furrow in the ventricular wall, out through the posterior ventricular wall, thence through the lower lobe of left lung, through the eleventh rib, embedding itself in the soft tissues of the back, about two inches to the left of the spinal column. The direction of the ball being oblique, the openings in the ventricular walls were valvular. This probably prevented more extensive hæmorrhage into the pericardium. There had been considerable hæmorrhage at the time of the wounding.

#### II. SCHULTZE'S METHOD OF TREATMENT OF ASPHYXIA OF THE NEW-BORN.

In a recent number of the *Glasgow Medical Journal*, Dr. Wm. L. Reid gives a brief account of the various methods of artificial respiration that have been suggested by different authors, and the results of comparison that have been made as to the relative efficiency in treating the condition of asphyxia in the new-born.

He quotes Carl Behn as giving the following three desiderata of artificial respiration:—First, ventilation of the lungs; second, excitation of the circulation; third, removal of foreign bodies from the air passages.

A series of experiments was made a few years ago by Dr. F. H. Champneys, Assistant Obstetrical Physician to St. George's Hospital, London, for the purpose of determining the relative efficacy of different modes of artificial respiration with reference to the first point—viz., the ventilation of the lungs, or the quantity of air taken into the lungs. He concluded that the well-known method of Silvester was the most efficient. Dr. Reid himself is a strong advocate of Schultzze's method, having observed its results as applied by Professor Schultzze himself at Jena, and having had quite extended experience with it in his own practice for the last six years.

The following brief description of the method is as given by Dr. Reid:—"The child is to be suspended a few inches from the floor, by the two index fingers placed in the axillæ from behind, the thumbs lying loosely over the front of the thorax, and the other fingers spread also loosely over the thorax behind, the head being supported against the edges of the ulnar bones. Without delay in this position, the child is swung sharply upwards until the

operator's arms are extended horizontally; then the upward movement is continued more gently, so as to bring the legs slowly past the perpendicular, and allow them to sink quietly against the front of the child's belly. The weight of the latter is now supported by the thumbs in front of the thorax, and the chest pressed on all round by the fingers, and its arms laid against its sides. This compression through the diaphragm below, and the fingers all round, causes aspirated fluids to flow freely from the mouth and nose. After being retained in this position a few seconds, the body is swung smartly down again into its former position, taking care that now there is no compression of the chest, either before or behind, but simply a suspension of the child on the fingers. During this movement the contents of the abdomen, partly by gravity and partly by centrifugal force, fly away from the diaphragm, and, dragging it down, enlarge the chest from below. At the same time the arms are separated from the sides, and by their muscular attachments drag the ribs upwards, and in this way air is sharply drawn into the lungs. These movements are continued every four or five seconds, unless when a considerable quantity of fluid continues to come from the mouth and nose, when the movement of expiration is on that account prolonged."

### III. STILL-BORN CHILD RESUSCITATED FORTY-FIVE MINUTES AFTER BIRTH BY ARTIFICIAL RESPIRATION.

In the *Lyon Médical*, Dec. 20, 1885, appears a report by Dr. J. Rendu of a case in which, after a rather tedious labour, the child was born apparently dead. At six o'clock the sounds of the foetal heart were regular and strong; at 6 20 the heart sounds were slower and less distinct; at 6.30 the sounds had ceased. Forceps were at once applied, and delivery was effected promptly and easily. The child gave no evidence whatever of life. It had a cadaveric pallor, and its pendent limbs offered no resistance. There was not the slightest respiration, nor could any sound of cardiac movement be heard. Yet no severe compression from forceps had occurred, nor was there any apparent injury of any sort to account for this condition. Mouth to mouth insufflation only served to distend the stomach. Immersion into warm water, frictions with alcohol, dropping water upon the chest, all were tried in vain.

The infant was then placed on a table, and a tube (Depaul's) was introduced into the larynx. By this means dilatation of the

chest was secured, and, alternating this dilatation with manual compression, very efficient artificial respiration was carried on for some minutes before any spontaneous effort at respiration was made by the child. This first evidence of life was manifested forty-five minutes after the birth of the child. Several minutes intervened before another voluntary effort was made, and the same again before the third. In this last interval pulsation became apparent at the epigastrium, and soon the heart action became natural. It was another hour before the surface assumed a natural colour and movements of the limbs were made. Two hours longer passed before respiration became free and natural.

#### IV. FEIGNED AND ARTIFICIAL ERUPTIONS OF THE SKIN.

Dr. W. A. Hardaway, in an interesting paper on this subject, read before the St. Louis Medico-Chirurgical Society on March 23rd, 1886, draws attention to this remarkable form of imposition; and, having detailed several cases that came under his observation, he proceeds to indicate some points which should be borne in mind in making the differential diagnosis between the so-called feigned and artificial eruptions and the substantive diseases of the skin. "In the first place," he says, "we should ascertain the antecedents of the patient, and take particular note of the present condition. Secondly, we should discover if the patient had ever suffered from any skin disease before, and whether the patient had been or was at the time making use, either locally or internally, of any medicinal preparation. We should compare the lesions present with other known eruptions of the skin, and make particular note of any anomaly. We should investigate closely the local expressions of the disease—for instance, as regards infiltration, itching, burning, &c. It must be borne in mind that most eruptions produced with intent to deceive generally occupy situations most accessible to the patient, and in the majority of cases, in right-handed persons, are apt to be on the left side of the body. It would be tedious to enter into the minute details of the differences between these feigned eruptions and those due to disease—as, for instance, how a pemphigus bleb will have a certain definite outline, and a bleb produced by nitric acid is apt to be jagged and irregular or linear; but I think enough has been given to show in a general way the cautions to be observed in coming to any conclusion in regard to a suspicious eruption. The late Dr. Tilbury Fox has pointed out that, so far as the simulated skin affections are concerned, they

generally are of the following forms:—The eruption is in erythematous or excoriated patches, such as may be produced by rubbing or by mustard; or bullous, or pustular, or more or less deeply ulcerative, and such as could be caused by the application of cantharides, croton oil, or some corrosive acid; or such an eruption as could be brought about by the constant forceful use of the finger-nails; or it is a pigmentation—*e.g.*, simulating chromidrosis, produced by black lead, candle black, grease, and other compounds."

#### V. HOMICIDE AND MUTILATION.

Dr. Matthew Hay reviews the case reported by Michelsen (Waldenburg) (in *Viertelj. für gerichtl. Med.*, Band xliii.), of an interesting and peculiar case of the murder of a young girl three years old. The child suddenly disappeared from her home, and her remains, much mutilated, were found two and a half weeks afterwards in an adjoining field. Another girl, ten years old, then confessed to having felled the child to the ground with a rake in a neighbouring potato-field. She went home and informed her own mother of what she had done, at whose suggestion she returned to the field, placed the child in a sack above some potatoes, and brought the sack on a wheelbarrow to her mother's house. The girl alleged that when her mother opened the sack the child regained consciousness and screamed, and that her mother then took the child to a cellar beneath and murdered it. This the mother altogether denied. Michelsen gave it as his opinion, after a necropsy, that the child had been suffocated by earth pushed into its mouth and throat, and that the body had been afterwards partly roasted and hewn into several pieces. The earth in the child's throat and trachea had the same composition, chemical and otherwise, as that of the potato-field, and was different from the earth taken from the cellar-floor. The mother was, however, convicted of murder and sentenced to death, the sentence being afterwards commuted to penal servitude for life. She then confessed that her daughter's statement was true. The latter had brought the child, as described, to the house, with an ugly gash on its head. To conceal the daughter's crime, she resolved to kill the child outright. She therefore took it to the cellar, and stuffed its mouth with the loose earth lying beside the potatoes—therefore earth from the potato-field. Here it lay for some days, when, as it began to decompose and smell, she placed the corpse over a fire in order to

burn it. This method of destruction proving imperfect, the body was hacked to pieces, and the main portion of it carried to the field in which it was eventually found.

#### VI. LUNG TEST IN INFANTICIDE.

Ebertz (*Viertelj. für gerichtl. Med.*, Band xliii.) reports a case of infanticide, where the usual lung-test of live-birth failed, although, from the confession of the mother, it was certain that the child had lived for some time (the time is not definitely stated) after birth. The mother admitted having killed the child by thrusting two pieces of rag into its mouth, and, fearing this was not sufficient, by afterwards driving the points of scissors into its neck and cutting the throat. The body was thrown, wrapped in a cloth, into water, where it was found four weeks later. The mother stated that the child neither moved nor cried after birth, although she knew it was alive from feeling the pulse in the arms. Ebertz believes the woman's account to be correct, although the usual evidence of live-birth at the necropsy was absent; no fragment of the lungs floated in the water. The rags were found stuffed into the throat, and the injury to the neck was extensive, embracing the trachea, but leaving the larger blood-vessels intact. The umbilical cord had been torn through, and was not tied. Ebertz is of opinion that the child was born in the condition of so-called apparent death—frequently observed by accoucheurs—and thus never breathed, although alive; or that it had breathed very lightly, but the lung had, at death, returned to an atelectatic condition. The latter view was suggested by a rounding of the edges of the lungs, and it is rendered possible by the comparatively recent experiments of Ungar, which go to prove that a new-born child may breathe for some time after birth, and yet its lungs may become, after death, completely atelectatic. The woman was convicted, and sentenced to one and a half year's imprisonment.

#### VII. SUICIDE AND ITS STATISTICS.

Dr. G. D'Arcy Adams reviews Morselli's recent work, as published in Nos. 4 and 5 of the *Giornale della Reale Società Italiana d'Igiene*, on the statistical laws of suicide, and summarises his conclusions in a late number of the *London Medical Record*, as follows:—

“1. Suicides are becoming more frequent, both in European and American countries, and this is especially the case since 1871.



2. In Europe the zone of maximum intensity of suicide crosses the Continent transversely ; it presents two chief foci, round which the majority of cases group themselves. These are Saxony and the Isle of France ; their populations, of Germanic race, pure or mixed with Celtic elements, afford the greatest number of suicides.

3. In all countries most cases occur in the best seasons of the year, in dry and warm weather ; fewest cases occur in cold and wet weather. Thus the maximum mean corresponds to June or May, the minimum to December.

4. As regards the religion of the victims, Protestantism affords the greatest number ; then, in decreasing order, Roman Catholicism, Greek or Orthodox Catholicism, Judaism, Mahomatanism, and Fetishism.

5. Large cities contribute a greater proportion of suicides than smaller towns, and these in their turn more than country districts.

6. Men are more prone to commit suicide than women, the proportion being 4 to 1.

7. The frequency of suicide increases regularly with age until old age is reached. Since the beginning of the century suicide in adolescence and childhood has continued to increase in frequency at a constant rate throughout the whole of Europe. In this, as in other things, the female sex is the most precocious.

8. Matrimony diminishes the inclination to suicide in both sexes ; widowhood and divorce lead to an extraordinary increase in the number of cases.

9. The agricultural and industrial classes show the least inclination to voluntary death ; suicide is most frequent in the cultured classes and the liberal professions. In all countries the largest quota of cases is furnished by the military among men, and by domestic servants among women.

10. Suicide by hanging is preferred by the Teutonic and Slavonic races, and generally prevails in winter and autumn, and in cities ; death by drowning, on the other hand, is most frequent in southern countries and in the Latin races, and generally in spring and summer, and in the country. Suicide by hanging and by firearms tends everywhere to increase, so that it may be held as an effect of civilisation.

11. Men in the countries of Northern and Central Europe prefer to kill themselves by hanging ; women, on the contrary, everywhere prefer drowning. Servants everywhere furnish the greatest number of deaths by poison, and soldiers almost invariably choose firearms.

12. With the development of intelligence and sensibility the more safe, rapid, and painless modes of death are more and more chosen."

## PART IV.

### MEDICAL MISCELLANY.

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*Reports, Transactions, and Scientific Intelligence.*

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#### ACADEMY OF MEDICINE IN IRELAND.

President—ROBERT M'DONNELL, M.D., F.R.S.

General Secretary—W. THOMSON, M.D.

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#### PATHOLOGICAL SECTION.

President—T. EVELYN LITTLE, M.D.

Sectional Secretary—J. B. STORY, M.B.

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*Friday, March 12, 1886.*

DR. E. HAMILTON and afterwards the PRESIDENT in the Chair.

#### *Dislocation of the Humerus with Fracture of the Great Tuberosity.*

The PRESIDENT read a communication on a case of dislocation of the humerus with fracture of the great tuberosity.

The CHAIRMAN said the case was interesting as showing a departure from the general rule of the small portion of the tuberosity being involved in these fractures. In other joints, when a fracture occurred, they saw scales of bone broken off instead of the ligaments and muscles giving way.

PROF. BENNETT said the specimen exhibited was of very great importance from its rarity and from the fact of its being completely dissected. Until recently it had been a difficulty to his mind as to whether or not they should divide fractures of the great tuberosity into two groups—viz. (1) fractures that occurred from direct injury to the tuberosity with subsequent sub-luxation of the humerus, and (2) complete dislocations with tearing of the attachments of the muscles. The case now adduced showed that they had dislocations completed by the tearing of the muscles and cases of primary fracture of the tuberosity. The case mentioned by the late Dr. Adams had been one of the greatest difficulties to his mind until he saw the present specimen. The association of a fracture of the glenoid with the other injury, and the fact of fragments of the great

tuberosity being comminuted, suggested the idea that the specimen was a case of fracture of the great tuberosity primarily, and that the dislocation was a secondary event. He could hardly conceive that such an amount of comminution as was shown in the Museum model specimen could have been produced by the mere tearing of the attachments of the muscles. That case, for a time, strongly inclined him to the opinion that there was only the one class of these fractures; but Dr. Little's case certainly justified the recognition of a second group of cases in which the tuberosity was torn off and not broken off by direct comminution. A point of great importance in the Museum specimen was that osseous union had taken place. The teaching of Professor Smith was that, when this accident occurred, union could be effected only by the ligaments. The Museum case, after the publication of his book, put all that aside. The evidence now proved that they should not accept the dictum that there was only a ligamentous union, but should direct their treatment towards obtaining osseous union if possible, such as had occurred in the Museum specimen. He had a case under treatment which was not a dislocation in the true sense, but a fracture of the great tuberosity with a sub-luxation inwards. The patient was an old woman, and yet a very complete union took place. Mr. Adams' case was published under a misnomer. It was recorded in the Pathological Transactions as a case of injury of the shoulder. In the records of this injury published in France and America, Mr. Adams' case was omitted, and Professor Smith's teaching—that only ligamentous union occurred—was recognised. Tearing of the tuberosities was, no doubt, of very rare occurrence.

The PRESIDENT (in reply) said he was struck by Professor Bennett's remark that, in the neighbourhood of the joint, portions of bone resembling scales were so often found broken off instead of the ligaments. It had struck him in several cases that the capsule of the joint had not been ruptured, and he had often known surprise to be expressed at that; but the breaking-off of the bone saved the ligaments from rupture. He was glad that Professor Bennett agreed with him in recognising the two classes of cases.

*Ulcerative Endocarditis limited to the Right Side of the Heart.*

DR. WALTER SMITH exhibited the viscera of a man, aged forty-four years. He had served in India for thirteen years, and enjoyed good health, except for several attacks of ague within the last two years. He was temperate. On December 23rd, 1885, he was seized with rigors, attended with cough, dyspnoea, loss of appetite, and disturbed sleep. He was admitted into Sir Patrick Dun's Hospital, on December 29th, in a drowsy, apathetic condition, and presented the physical signs of pneumonia of the left lung. T. 103.2°; R. 30; P. 100. Urine contained a little albumen, was rich in urobilin, and deficient in chlorides at first.

Heart's sounds normal. Convalescence from the acute attack apparently set in on the ninth day, but four days later the temperature rose to  $104.6^{\circ}$ , and evidence of a fresh pneumonic attack in the left lung was found. Then a few days subsequently pleuro-pneumonia of the right side declared itself, with abundant, tenacious, rusty sputum. Diarrhoea afterwards set in; he became extremely weak, and died quietly on Feb. 20th. Eleven days prior to death a systolic blowing murmur developed towards the apex of the heart and persisted, and the existence of an ulcerative endocarditis was conjectured.

*Post-mortem examination*:—Liver, 71 oz.; nutmeggy-spleen, 15 oz., almost diffuent; several red infarcts along its thin edge; section speckled with numerous red and dark spots; kidneys pale, full size and apparently healthy; several pints of fluid in right pleura; right lung, 38 oz.; left lung, 24 oz.; left pulmonary artery, at its bifurcation, blocked by a large yellow clot, intimately adherent to the wall of the vessel; numerous firm thrombi in the smaller branches of the pulmonary artery in each lung; several lumpy patches of consolidation in each lung, and one or two sharply-defined infarcts; no trace of pericarditis; heart, 13 oz., left chambers healthy; aortic and pulmonary valves competent and normal in appearance; tricuspid valve extensively diseased—it was covered with enormous ( $1\frac{1}{2}$  inch) cauliflower excrescences and vegetations, some hanging by a narrow pedicle close to the free edge of the valve; close to one curtain of the valve was a ragged cavity in the heart muscle, about  $\frac{1}{2}$  inch in length; surface rough and uneven; a patch of granular exudation upon the endocardium of the right ventricle; no disease of the pulmonary artery; a number of small firm thrombi were entangled in the recesses of the muscoli pectinati of the right ventricle.

The case was obscure in its origin and clinical course, and the limitation of endocarditis to the right side of the heart is noteworthy.

#### *Endocarditis.*

DR. C. J. NIXON communicated another case of endocarditis. He said the specimen did not present any features of peculiar interest pathologically or clinically, but was merely a good example of the changes that were met with in endocarditis. The viscera were removed from a boy, aged twenty, who was admitted into the Mater Misericordiæ Hospital, on the 20th of December, 1885. Six months previously he had an attack of rheumatic fever in which his heart was affected. Afterwards he complained of some difficulty of breathing, especially on exertion; but it was only a fortnight before his admission to hospital that serious symptoms presented themselves. He then got a rather sharp attack of hæmoptysis, and came to the hospital for treatment. He was remarkably anæmic; had considerable dyspnoea and orthopnoea; his urine was small in quantity and slightly albuminous; and he had general anasarca and a moderate

amount of ascites. On examining the heart he found the usual signs of well-marked mitral regurgitation. Occasionally, in addition, a feebly-pronounced presystolic murmur was heard. In the course of the case the symptoms that usually developed towards the late stages of mitral disease were present; the most marked symptom that called for treatment being sleeplessness. Towards the end the boy passed into a condition of drowsiness which merged into coma, and he died on the 30th of December, ten days after his admission.

His heart was enlarged and weighed 14 ounces. On the left side could be observed well-marked evidences of mitral valvulitis of a rather extensive character. The anterior flap of the mitral valve was partially disconnected from its chordæ tendineæ, and, at the extremities of the ruptured cords, a precipitation of fibrin had taken place. In one place the precipitation of fibrin was attended with calcification. On opening the left auricle they found on the posterior aspect of it a very well-marked change in the endocardium. The surface of the endocardium looked like very coarse velvet pile or plush, and there was a considerable deposition of lymph exudation upon it. This, on examination with the microscope, presented the ordinary appearance of embryonic tissue. He looked carefully for the existence of micrococci, but failed to find any. The chief evidences of disease were the marked changes in the mitral valves, the rupture of the chordæ tendineæ, and the well-marked endocarditis in the usual position on the posterior aspect of the auricle. The right side of the heart presented nothing unusual, except that the right ventricle was slightly dilated. On the posterior wall of the right auricle, as far as the base of the auricular appendix, they had well-marked evidence of limited pericarditis—roughness and opacity of the pericardium. This did not present the appearance of the usual milk spots, found so frequently on the anterior aspect of the heart. The patient had, before admission and during the time he was under observation in the hospital, attacks of hæmoptysis; and it was suspected that they would find hæmorrhagic infarction of the lungs. That was verified by the *post mortem*, which showed, in various parts of the lungs, well-marked evidence of hæmorrhagic infarction. The branches of the pulmonary artery were dissected by Dr. Barningham, and one of the branches leading to a spot of hæmorrhagic infarction was found to be filled with a distinct plug. He (Dr. Nixon) had been for a long time in the habit of teaching that they might regard heart disease, so far as regarded the development of its symptoms, as eminently paroxysmal. They found that, in a patient who had chronic mitral or aortic valve disease, the evidences of those diseases were for a time by no means prominent, and the patient followed his usual avocations, did not suffer from breathlessness, and ate and slept well. Then the scene would change, and he would have sleepless nights and dyspnoea on the slightest exertion; after a time those symptoms would pass away,

and the patient would be restored to his former condition, the symptoms of the disease again becoming trivial in character. They could only account for these changes of a paroxysmal character by assuming that, in the course of chronic endocarditis, they had supervening at different times acute accessions of endocarditis, the acute endocarditis supervening on the old standing disease of the valves; and, as it manifested itself, corresponding symptoms were developed.

The **PRESIDENT** remarked that whether the disease occurred on the right or the left side of the heart a vegetative affection of the endocardium was present. As to the specimen which he himself found in the Anatomy Room, and to which Dr. Smith had referred, it had, while recent, a most extraordinary similarity, as regarded the condition of the valves, to the specimen which Dr. Smith had shown. Several of the vegetative growths which it then exhibited had since been knocked away.

**DR. NIXON** said that in Dr. Smith's case the extent to which warty vegetation existed in connection with the tricuspid valve, while the valve segments themselves seemed healthy and unaltered in appearance, was remarkable. The specimen had no doubt been in spirit for some time, but he had not seen in it any of the usual signs of valvulitis. There did not seem to be any thickening of the segments of the tricuspid valve, and their margins looked remarkably healthy; so that the specimen did not present any appearance of ulcerative endocarditis. The specimen was of considerable interest, because he had never seen—and he had some experience of pathological changes in hearts—any case of primary endocarditis on the right side. He had seen it—as most of the members of the Society had—following affections of the left side, the common condition being tricuspid stenosis following mitral stenosis. They were all familiar with the history of endocarditis of the right side, as being usually a disease of foetal life; it might arise from some defect or malformation of the heart—with which he believed it was usually associated—or from some alteration or interference with the circulation, through the umbilical arteries, or through the placenta. The conditions which influenced the tension of the vessels after birth were likely to produce, from increased pressure, endocarditis of the left side of the heart; whereas in the foetus the conditions affecting arterial tension were conditions occurring to the circulation through the umbilical arteries or the placenta, in consequence of which they were likely to have the foetal endocarditis on the right side of the heart. It would not be right to deny that in Dr. Smith's case there was endocarditis of the right side of the heart. The only explanation of it that occurred to him was that in the case they had conditions of circulation through the lungs which favoured a slow movement of blood through the right side of the heart; and in consequence of this—just as in cases of mitral stenosis—coagulation and deposition of fibrin in the muscular trabeculae on the right side

of the heart took place. In this case the deposition of the fibrin seemed to have set up the endocarditis which was stated to have existed. He thought there was no evidence that the disease had any septic origin. He did not see what explanation could be given of the occurrence of the endocarditis, which would strictly limit itself to the right side of the heart, except in connection with the deposition of fibrin in the valves or in connection with the muscular trabeculæ. With respect to his own case, he found, on making the *post mortem* examination—as he had frequently noted in other cases—a limited amount of pericarditis on the posterior aspect of the right auricle. He had observed this in cases of pneumonia, especially in the suppurative stage; and it was the only part of the pericardium that was involved.

DR. HENRY KENNEDY said the late Dr. Graves had given a remarkable case of acute disease attacking the right side of the heart, and in which the pulmonary valves were found to be densely covered with lymph. Dr. Smith's case appeared to be one of disease of both lungs and heart. The frequently-recurring rigors had been set down as indicative of this particular form of ulcerative endocarditis. The extent to which lymph had been poured out was very remarkable—he had seen nothing like it. As for Dr. Nixon's view of recurring pericarditis going hand in hand with an increase of the symptoms when once the disease was established, he thought it questionable. If that were so, many of the cases that they knew lasted for years could scarcely have held out. There were paroxysmal symptoms in other diseases in which there was no pouring out of lymph, and he did not see why the same thing should not occur in the case of the heart. The occurrence of pneumonia with such a state of the heart was novel. The state of the spleen—though they did not know the functions of that organ—must have helped to impair the deceased's constitution, and so led to the disease of which he died.

MR. DOYLE asked did Dr. Smith make a microscopic examination of any of the products found in the heart he had submitted. If he mistook not, it had been stated that the case was first looked on as one of ordinary croupous pneumonia, which was supposed to depend on micrococci; and if that were so the disease might have been introduced into the heart. Professor Purser had brought an able paper before that Section on a case of diphtheritic endocarditis, in which micrococci were found; and what first made him make up his mind as to the diagnosis was the development of a murmur which occurred shortly before the man's death. With respect to Dr. Nixon's case, the rupture of the chordæ tendinæ appeared to him (Mr. Doyle) to have been *post mortem*. The two cases showed how very unreliable murmurs were. In Dr. Nixon's case there were all kinds of murmurs occurring at different times in the progress of the disease. Did Dr. Nixon make any microscopic examination of the thrombus in the lungs?

DR. SMITH, in reply, said as yet there had been no microscopic examination of the tissues in his case. The lungs were not now in a state in which examination would be of any use, but he intended to submit the heart to Professor Purser. He had only to add that the general aspect of the man's case, his temperature chart and so forth, were certainly not in disaccord with the course of what was described as the typhoid form of ulcerative endocarditis. The non-discovery of any evidence of septic infection was certainly not of itself a reason for hesitating to make a diagnosis implying the existence of such infection. All physicians and surgeons of experience knew that undoubtedly cases of blood-poisoning, pyæmia, or septicæmia occurred, in which the people died, and in which it was yet impossible to find any cause for the pyæmia. Therefore the non-discovery of the source of blood-poisoning did not exclude the diagnosis of septic ulcerative endocarditis. Besides, ulcerative endocarditis did not necessarily involve a septic cause. Acute inflammation of the valves might cause the destruction without the aid of germs from other quarters.

DR. NIXON, in reply, said one thing made against Dr. Smith's view that the case was one of septic endocarditis was—that there was no evidence of septic thrombi in the lungs, or of suppuration occurring. He did not consider that he had demolished the theory of endocarditis in Dr. Smith's case.

#### *Cirrhosis of the Liver.*

MR. STORY showed, on behalf of DR. JOHN WILLIAM MOORE, a specimen of hypertrophic chronic interstitial hepatitis (cirrhosis of the liver), fatal through hæmatemesis. The patient was a foreman painter, aged fifty-three years. There was a history of "nipping" extending over many years. There were large deposits of fat round the viscera and in the subcutaneous areolar tissue. There was moderate ascites. The stomach and intestines were deeply stained with disintegrated blood extravasations and clots. The liver was extremely granular, and was of the character called "hobnail." It was of very dense consistence, cut like leather, and weighed 64 ounces. There were evidences of peri-hepatitis and perisplenitis. The kidneys and other viscera were blanched. The hæmatemesis set in on the 3rd of March, 1886, with great violence, and recurred again and again, finally causing death on the 10th of March. There were tarry stools from time to time.

The Section then adjourned.



## SUB-SECTION OF STATE MEDICINE.

Chairman—A. H. JACOB, M.D., F.R.C.S.I.

Secretary—H. C. TWEEDY, M.D. Univ. Dubl., M.K.Q.C.P.

*Thursday, April 8, 1886.*

DR. A. H. JACOB in the Chair.

*Sanitary Conditions concerning Dublin Dairy Yards.*

DR. C. F. MOORE read a paper offering considerations and suggestions in reference to the sanitary condition of Dublin dairy yards.

The supply of pure milk, free from a suspicion of adulteration or of infection, is a matter of great importance, and has been dealt with by the Legislature in a very practical way.

It is well worth while, however, to assist in directing dairy men and others interested in cattle-keeping how to avoid the evils which have of late given Dublin an unenviable notoriety. It is unimportant here to trace the origin of pleuro-pneumonia to its Eastern source; the object of this paper is to show that much may be done to lessen the likelihood of its origination *de novo* if it is so, or its continuance in a congenial soil.

Some have considered the importation of the disease as due to free trade; however that may be, self-interest should arm us in the endeavour to combat successfully with so serious a foe, and while doing so to remember that we are benefiting the health of our people in many directions.

The steps taken by the Legislature and Executive against cattle disease in every form embraces several important procedures.

Nor are the means heretofore adopted in Dublin and the suburbs a whit too powerful; pleuro-pneumonia still thrives on its victims, and the ratepayers still suffer as well as the health of the people. The present paper is an endeavour to point out the best interests of the producer as well as of the public generally. The steps taken in other countries to several of the same ends is pointed out, and the writer hopes that progress already made will be continued until the opprobrium alluded to is removed, and the public health also vastly improved.

DR. FRAZER said it was easy to understand the tendency of a disease to spread, but was there no origination of the disease—were isolated herds of cattle free from it? It was worth while considering whether there was any connection between the spread of that disease and the fearfully rapid forms of pneumonia in human beings during the past couple of years in Dublin. No doubt the grazing of cattle left the seeds of the disease in the grass. The carcasses of cattle slaughtered for disease should be effectually destroyed or cremated.

REV. CANON BAGOT, as a visitor, said it ought to be generally known, with regard to the milk-supply of a large city like Dublin, that though there were constant prosecutions for adulterated milk, yet the law had not yet defined what was pure milk. Most analysts adopted the Somerset House standard, but the statute had not fixed that or any other standard. In his opinion, the Somerset House standard was too low—namely, 10 per cent. solids and 90 per cent. water. The American standard was nearer the mark—12½ per cent. solids and 87½ per cent. water; and the American Act prohibited the feeding of cattle whose milk was to be used for human food on any fermented food except silage. So that the Americans would consider the milk of cattle fed on grains adulterated. In one year there would not be a dairy yard within the borough boundary if only the orders already promulgated were strictly and impartially carried out. As an instance, he had himself two or three registered milk shops, and during the last three years they were never once inspected to see whether they were in accordance with the regulations. The necessity of providing a market for the brewery grains by keeping dairy yards within the city no longer held good, the grains being carried all over the country by rail. Dublin was the centre of pleuro-pneumonia for the United Kingdom. There was no doubt the disease could be stamped out, as it had been done over and over again in England. But there was another aspect of the sanitary question as regards cattle worthy of consideration. He went into the hold of one of the cattle-carrying steamers about an hour after starting from North-wall, and he found the thermometer at 160°. Taking this to be the case in winter, when the cattle were taken out and placed in railway wagons and whirled through the country, was it any wonder that, though starting free from one disease and declared free, pleuro-pneumonia was thus created by the journey? Cattle killed in a high state of fever might possibly create disease in the people who partook of it. By killing the cattle where they were reared, the meat could be sent by sea or rail in a wholesome state, and that was the legitimate way of transporting fat meat, instead of sending the animals alive. In Denmark cows were fattened, and the manure was never removed from the byre for the six or seven months they were kept there; and though the attendants generally slept in the cowhouses, there was no complaint of their being unhealthy. There was no smell of decomposing matter. Entering a cowshed the only smell was of ammonia. He had been in a Denmark dairy where 300 cows were thus kept.

DR. JOHN WM. MOORE had for several years passed daily from the Meath Hospital to Cork-street Fever Hospital, in the region of dairy yards. From external appearance some of those dairies were anything but suitable places for the preservation of milk, and he had had from the registered dairy a succession of cases of typhoid fever, and from another

an outbreak of typhus. Through the walls of the dairy yard liquid sewage matter oozed. In frost ice took up the colouring matter with curious effect, and the asphalted pavement broke up periodically.

*The Working of the Contagious Diseases Acts.*

The CHAIRMAN (DR. JACOB) read a paper dealing with the working of the Contagious Diseases Acts.

REV. CANON BAGOT concurred with Dr. Jacob as to the enormous benefit of the Acts. As an illustration, in a Kildare County Infirmary, before the Acts came into operation at the Curragh, there was a ward for male syphilis cases, of which there were constantly seven or eight, but within six months after the Act was applied the ward was closed and had remained closed since. Now, however, the governors of the hospital feared it would become necessary to reopen it. With regard to the moral view the whole question was begged; because, going a little further with it, soldiers must be allowed to have wives. It seemed, therefore, a hard case to plant a large number of men at the Curragh and a large number of prostitutes in their train, and then leave the civil population unprotected. He was always a warm advocate of the Act, and thought agitation for its continuance, even now, would be useful.

MR. DOYLE considered the Act a most oppressive one, and if applied at all it should be equally to both sexes. He believed the medical profession knew no more about syphilis now than was known 200 years ago. Was any medical man able to say that a given person was free from disease or incapable of communicating it? Why should medical men be so dishonest as to allege that even on the most minute examination of a woman they could make it out? On this ground it was a shame for the profession to have anything to say to the Act. It had been established that the emanations of a woman suffering from the disease were capable of producing one form of disease in one individual and a different form in another. Medical men were set down as honourable, but in this matter the monetary consideration was the groundwork of their advocacy.

DR. FRAZER concurred as to the difficulty of determining either syphilis or gonorrhœa in the female. What could be said for such a lopsided Act of Parliament as that which let the man go free? Laying aside all other arguments, if the Act was to obtain the sanction of honest, fair-thinking men, it must impose equal penalties on males and females.

DR. TAYLOR said the argument showed that the Act, as far as it went, was a good one, and that it did not go far enough.

DR. JOHN WM. MOORE was astonished at the logic of Mr. Doyle and Dr. Frazer. The Contagious Diseases Acts were not introduced with the object of examining women at large, but with the view of controlling damage done by a certain class of women who, in accordance with the Acts, acknowledged themselves to be prostitutes. Under such circum-

stances the Acts could not apply to women in general, but only to women who declared themselves to be prostitutes. A given soldier could not be looked upon as a prostitute. In all probability, the soldier who would subject himself to examination would be an unmarried man who would associate with prostitutes.

REV. CANON BAGOT understood that in working the Act every regiment was medically examined, and the men obliged to declare if the women they had had connexion with were at the Curragh.

DR. ROBERT MONTGOMERY repudiated Mr. Doyle's aspersions on the profession. Some of the highest medical men in England, who could possibly have no other motive than the public good, advocated the continuance of the Acts. It was new to him that experts had a difficulty in diagnosticating the disease. Prostitutes were seducers and should be looked after.

DR. CHARLES MOORE mentioned the case of a lad, fourteen or fifteen years of age, who had been seduced by a prostitute. Such women were under the impression that by giving the disease to another they got rid of it themselves.

The CHAIRMAN (DR. JACOB) replied. Prostitutes were a class of persons who were driving a trade in prostitution, and it was the duty of the State to see that the persons engaged in that trade pursued it with the least disadvantage to the general public. It was desirable to put pressure on men to confess the existence of disease when they possessed it; but he protested against the view that man as man or woman as woman should be subjected to the same restrictions as prostitutes. As to discriminating the existence of venereal disease, there were some obscure forms of it that there was considerable difficulty in diagnosticating by observation; but, like every other disease under the sun, there were twenty other forms of it that were perfectly obvious to any inspecting surgeon, and these ought to be nipped in the bud. The law ought to provide machinery for stopping those obvious cases. Therefore Mr. Doyle's argument was good only so far as regarded the percentage of those forms of the disease that could not be readily diagnosticated. Without palliating prostitution, he contended that the Acts ought to be maintained for the advantage of the women themselves, as giving them the means of having their diseases examined into in the early stages and cured. From his observation those women went voluntarily to be examined for their own protection. There was no dragging or oppression. The good results which accrued to the army and navy should not be sacrificed for the sake of any prurient sentimentalism. He protested against the slur which had been cast on an honourable profession, whose members had no interest in the matter to the extent of sixpence save to do what was right and philanthropic.

The Sub-Section adjourned.

# SANITARY AND METEOROLOGICAL NOTES.

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## VITAL STATISTICS

*For four Weeks ending Saturday, April 24, 1886.*

The deaths registered in each of the four weeks in the sixteen principal Town Districts of Ireland, alphabetically arranged, corresponded to the following annual rates per 1,000 :—

Towns	Weeks ending				Towns	Weeks ending			
	April 8.	April 10.	April 17.	April 24.		April 8.	April 10.	April 17.	April 24.
Armagh -	41·3	31·0	15·5	10·3	Limerick -	23·3	23·3	22·9	23·3
Belfast -	38·1	21·2	28·2	21·9	Lisburn -	14·5	9·7	14·5	14·5
Cork -	31·8	20·1	14·9	22·1	Londonderry	21·4	21·4	23·2	19·6
Drogheda	46·5	12·7	21·1	12·7	Lurgan -	41·0	15·4	20·5	35·9
Dublin -	32·9	31·9	24·7	26·0	Newry -	17·6	38·6	14·0	14·0
Dundalk -	30·6	4·4	4·4	30·6	Sligo -	19·2	9·6	19·2	9·6
Galway -	30·3	20·2	30·3	16·8	Waterford -	23·2	37·0	48·6	25·5
Kilkenny	16·9	29·6	12·7	8·5	Wexford -	21·4	29·9	21·4	8·6

In the week ending Saturday, April 3, the mortality in twenty-eight large English towns, including London (in which the rate was 22·0), was equal to an average annual death-rate of 22·2 per 1,000 persons living ; in Glasgow the rate was 25·5 ; and in Edinburgh 19·8. The average annual death-rate represented by the deaths registered in the sixteen principal town districts of Ireland was 31·3 per 1,000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 2·1 per 1,000, the rates varying from 0·0 in nine of the districts, to 10·3 in Lurgan ; the 8 deaths from all causes registered in that district comprise 1 from measles and 1 from scarlatina. Among the 141 deaths from all causes registered in Belfast are 1 from scarlatina, 1 from typhus, 3 from whooping-cough, 2 from

simple continued and ill-defined fever, 1 from enteric fever, and 4 from diarrhoea; and the 21 deaths in Limerick comprise 2 from whooping-cough.

In the Dublin Registration District the births registered during the week amounted to 257—141 boys and 116 girls—and the deaths to 233—120 males and 113 females.

The deaths represent an annual rate of mortality of 34·4 in every 1,000 of the estimated population; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 32·9 per 1,000.

Twenty deaths from zymotic diseases were registered, being one under the number for the preceding week, and 11 below the average for the 13th week of the last ten years; they comprise 3 from scarlet fever (scarlatina), 1 from typhus, 6 from whooping-cough, 1 from cerebro-spinal fever, 4 from enteric fever, 1 from diarrhoea, &c.

In the week ending Saturday, April 10, the mortality in twenty-eight large English towns, including London (in which the rate was 19·9), was equal to an average annual death-rate of 20·1 per 1,000 persons living; in Glasgow the rate was 24·6; and in Edinburgh 17·8. The average annual death-rate in the sixteen principal town districts of Ireland was 26·1 per 1,000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 2·2 per 1,000, the rates varying from 0·0 in Londonderry, Waterford, Wexford, Dundalk, Sligo, and Lurgan, to 10·5 in Newry; the 11 deaths from all causes registered in the last-named district comprise 2 from whooping-cough and 1 from diarrhoea. Among the 90 deaths from all causes registered in Belfast are 1 from typhus, 2 from whooping-cough, 2 from diphtheria, 1 from ill-defined fever, and 1 from enteric fever; among the 31 deaths in Cork are 2 from scarlatina; and the 21 deaths in Limerick comprise 1 from each of the following diseases, viz., typhus, whooping-cough, and diarrhoea.

In the Dublin Registration District the births registered during the week amounted to 184—109 boys and 75 girls—and the deaths to 223—112 males and 111 females.

The deaths represent an annual rate of mortality of 32·9 in every 1,000 of the estimated population; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 31·9 per 1,000.

Eighteen deaths from zymotic diseases were registered, being 11 below the average for the corresponding week of the last ten years, and 2 under the number for the week ended 3rd instant; they comprise 2 from scarlet fever (scarlatina), 11 from whooping-cough, 3 from enteric fever, &c.

In the week ending Saturday, April 17, the mortality in twenty-eight large English towns, including London (in which the rate was 19·8), was equal to an average annual death-rate of 20·3 per 1,000 persons living; in Glasgow the rate was 22·8; and in Edinburgh 18·0. The average annual death-rate in the sixteen principal town districts of Ireland was 24·2 per 1,000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 1·9 per 1,000, the rates varying from 0·0 in eleven of the districts to 4·6 in Waterford; the 21 deaths from all causes registered in that district comprise 2 from diarrhoea. Among the 120 deaths from all causes registered in Belfast are 5 from whooping-cough, 1 from enteric fever, and 2 from diarrhoea; and the 23 deaths in Cork comprise 2 from measles, 1 from scarlatina, and 1 from whooping-cough.

In the Dublin Registration District the births registered during the week amounted to 194—104 boys and 90 girls—and the deaths to 169—88 males and 81 females.

The deaths represent an annual rate of mortality of 25·0 in every 1,000 of the estimated population; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 24·7 per 1,000.

Eighteen deaths from zymotic diseases were registered during the week, being equal to the number for the preceding week, and 9 under the average for the 15th week of the last ten years; they comprise 2 from scarlet fever (scarlatina), 5 from whooping-cough, 3 from diphtheria, 3 from enteric fever, 2 from diarrhoea, 1 from dysentery, &c.

In the week ending Saturday, April 24th, the mortality in twenty-eight large English towns, including London (in which the rate was 18·4), was equal to an average annual death-rate of 19·8 per 1,000 persons living. In Glasgow the rate was 25·0; and in Edinburgh 17·0. The average annual death-rate in the sixteen principal town districts of Ireland was 23·1 per 1,000 of the population.

The deaths from the principal zymotic diseases in the sixteen districts were equal to an annual rate of 1·2 per 1,000, the rates varying from 0·0 in ten of the districts to 8·5 in Drogheda; the 3 deaths from all causes registered in that district comprise 2 from diphtheria. Among the 93 deaths from all causes registered in Belfast are 1 from scarlatina, 2 from whooping-cough, 1 from diphtheria, and 1 from enteric fever; and the 34 deaths in Cork comprise 1 from enteric fever and 2 from diarrhoea.

In the Dublin Registration District the births registered during the week amounted to 182—92 boys and 90 girls—and the deaths to 185—89 males and 96 females.

The deaths represent an annual rate of mortality of 27·3 in every

1,000 of the estimated population; omitting the deaths of persons admitted into public institutions from localities outside the district, the rate was 26·0 per 1,000.

Seventeen deaths from zymotic diseases were registered, being 13 below the average for the corresponding week of the last ten years, and 1 under the number for the week ended 17th instant; they comprise 1 from relapsing fever, 4 from whooping-cough, 1 from cerebro-spinal fever, 2 from simple continued and ill-defined fever, 1 from enteric fever, 1 from dysentery, 3 from erysipelas, &c.

### METEOROLOGY.

*Abstract of Observations made in the City of Dublin, Lat. 53° 20' N., Long. 6° 15' W., for the Month of April, 1886.*

Mean Height of Barometer,	-	-	-	29·909 inches.
Maximal Height of Barometer (on 15th, at 9 a.m.)	-	-	-	30·404 „
Minimal Height of Barometer (on 8th, at 9 a.m.),	-	-	-	29·022 „
Mean Dry-bulb Temperature,	-	-	-	45·5°.
Mean Wet-bulb Temperature,	-	-	-	42·6°.
Mean Dew-point Temperature,	-	-	-	39·3°.
Mean Elastic Force (Tension) of Aqueous Vapour,	-	-	-	·243 inch.
Mean Humidity,	-	-	-	79·5 per cent.
Highest Temperature in Shade (on 27th),	-	-	-	64·8°.
Lowest Temperature in Shade (on 11th),	-	-	-	31·8°.
Lowest Temperature on Grass (Radiation) (on 11th),	-	-	-	26·9°.
Mean Amount of Cloud,	-	-	-	54·0 per cent.
Rainfall (on 15 days),	-	-	-	1·524 inches.
Greatest Daily Rainfall (on 4th),	-	-	-	·374 inch.
General Directions of Wind,	-	-	-	N.E., E., W.

### Remarks.

Opening with unsettled, stormy, and showery weather, this month proved finally to be both dry and cold—only a little more than half of inch of rain (·550 inch) falling after the 9th on but six days, and polar (E. to N. W.) winds blowing almost constantly during the same period. Previously, strong S. to W. winds had prevailed, with higher but unstable temperature and frequent showers—these conditions being brought about by the passage northeastwards along our Atlantic coasts of a series of extensive and deep atmospherical depressions. In Dublin the mean temperature (46·3°) was lower than that recorded in the month of April except on three occasions in the twenty years, 1865–84—viz., in 1877 (46·1°), in 1879 (44·5°)—the lowest mean temperature yet observed in April—and in 1881 (45·6°). In 1884 the mean temperature was one-tenth of a degree higher (46·4°) than it was in the present year.



The mean height of the barometer was 29·909 inches, or 0·052 inch above the average value for April—namely, 29·857 inches. The mercury rose to 30·404 inches at 9 a.m. of the 15th, and fell to 29·022 inches at 9 a.m. of the 8th. The observed range of atmospherical pressure was, therefore, 1·382 inches—not much less than an inch and a half. The mean temperature deduced from daily readings of the dry bulb thermometer at 9 a.m. and 9 p.m. was 45·5°, or 4·3° above the value for March, 1886; that calculated by Kaemtz's formula—viz.,  $\text{min.} + (\text{max.} - \text{min.} \times .41) = \text{Mean Temp.}$ —from the means of the daily maxima and minima was 45·1°, or 2·0° below the average mean temperature for April, calculated in the same way, in the twenty years, 1865–84, inclusive (47·1°). The arithmetical mean of the maximal and minimal readings was 46·3°, compared with a twenty years' average of 48·1°. On the 27th the thermometer in the screen rose to 64·8°—wind E.S.E.; on the 11th the temperature fell to 31·8°—wind N. The minimum on the grass was 26·9° on the same date. The diurnal range of temperature was large on some occasions—notably on the 1st, 7th, 12th, and 30th. The rainfall was 1·524 inches, distributed over 15 days. The average rainfall for April in the twenty years, 1865–84, inclusive, was 2·029 inches, and the average number of rainy days was 15·0. The rainfall, therefore, was decidedly below, while the rainy days were exactly equal to, the average.

Sleet fell on the 6th. Hail was noted on the 6th, 8th, 9th, 10th, 17th, and 29th. The air was more or less foggy on the 11th, 16th, 24th, 27th, and 30th. Solar halos were observed on the 12th and 17th. High winds prevailed on as many as nine days, of which six fell within the first week.

Until Saturday, the 10th, atmospherical pressure was lowest over the Atlantic between the British Islands and Iceland, as well as over the Norwegian Sea, and a number of depressions travelled from S.W. to N.E. along the shores of northwestern Europe. High winds, showers, and bright intervals prevailed at this time. The most important of these depressions came suddenly to the N.W. of Ireland during the night of the 6th–7th. Its rate of motion was immediately checked, so that its centre lay over Sutherland at 8 a.m. of the 8th, and had only reached the Shetlands twenty-four hours later. In this depression the barometer sank below 28·60 inches. Many subsidiary disturbances accompanied it, and the weather was consequently very unsettled and rough, with cold showers of hail and sleet in many parts of the United Kingdom, as the wind veered to W. and N.W.

In the course of the week ending Saturday the 17th, the weather was cold and rather unsettled. Northerly (N.W. to N.E.) winds of moderate strength prevailed over the British Islands during the entire period, but on the whole the weather was brighter and drier in Ireland than in Great

Britain, especially towards the close, when the N.E. wind freshened with cold showers in the E. and S. of England.

The weather of the week ending the 24th was generally fair, with low temperatures until the end. On Friday, the 23rd, the sky became gloomy in Ireland with a freshening easterly wind, and at night a fall of rain occurred. Next day, a spell of warmth occurred in England, the thermometer rising to 68° at Cambridge, 69° at Loughborough and Oxford, and 70° in London.

Very important and decided changes occurred in the weather of the last week, the conditions being sometimes very springlike and at other times quite winterly. At first the weather was fine and bright, particularly on Easter Monday, the 26th; and temperature rose daily until Tuesday afternoon, when it reached 64·8° in Dublin, 67° in London, 71° at Parsonstown and some English stations, and 72° at Leith. On this same day, a N.E. wind, low temperature (36°) and sleet and snow prevailed in the Shetland Isles. The barometer now rose in the north, so that the cold N.E. wind spread steadily southwards, displacing the warm air-current just referred to, and bringing with it cold rain and sleet to many places. In Dublin, a heavy fall of cold rain occurred on Wednesday afternoon, the 28th, and next day rain, sleet, and hail fell heavily in London, reducing the temperature to 36° in the shade. Night frosts now occurred in most parts of the country—the thermometer in the screen falling to 28° at Parsonstown and 33·2° in Dublin on the morning of the 30th.

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#### KRULL'S METHOD OF TREATING CATARRHAL JAUNDICE.

M. R. LONGUET (*Union Médicale*) gives an account of this treatment and of Löwenthal's recent confirmation of its efficacy. It seems that Krull published an account of his method in 1877, but that it excited little attention until Löwenthal took it up. It consists simply in the administration of enemata of cold water. The first injection, of one or two quarts, at a temperature of 59° F., is thrown in gently, and retained as long as possible. On the succeeding days an enema is given every morning, the temperature being gradually increased to 71·6° F., which is not exceeded. The cure is generally accomplished by the fourth day, and in no instance have more than seven injections been found necessary. No failures are mentioned, although several of the cases were of long standing, and had resisted the most varied treatment, including the use of that *ultima ratio* of the Germans, Carlsbad water. No medicine is allowed to be taken, and the diet is restricted to vegetables. Löwenthal, who used injections somewhat colder than those mentioned, tried the method in forty-one cases, and he absolutely confirms Krull's report of its efficiency.—*N. Y. Med. Jour.*

## PERISCOPE.

### THE INFLUENCE OF KAIRIN, THALLIN, HYDROCHINON, RESORCIN, AND ANTIPYRIN ON THE HEART AND BLOOD-VESSELS.

CHEMISTS, for a number of years, have been industriously experimenting, hoping to find a way to produce quinine artificially. The result has been the discovery of a number of substances, some of them belonging to the phenol series of organic compounds, and possessing to an eminent degree the power of reducing hyperpyrexial temperatures. Of these, kairin, thallin, hydrochinon, resorcin, and antipyrin, have all been found to reduce abnormal temperatures to a greater or less degree, in almost all febrile disorders promptly, though perhaps not permanently. An experimental inquiry into the probable relations of these new antipyretics to the circulatory apparatus has been made by Dr. H. G. Beyer, and the results, which he gives in an elaborate article on the subject in the April number of *The American Journal of the Medical Sciences*, justify the attempt to solve the problem. The experiments have been arranged into two groups:—I. Experiments on the work done by the heart when isolated from the central nervous system. II. Experiments on the blood-vessels; on the flow through the vessels of animals the brains and spinal cords of which had been destroyed; on the lingual vessels of curarised frogs. In addition to this, a short account of the influence of these drugs upon the corpuscular elements of the blood and the coagulation of blood is given. Dr. Beyer's experiments show that kairin reduces temperature, both by diminishing heat production and by increasing heat radiation. The distinctive influence it exerts on the red blood-corpuscles, however, and the weakening effect upon the heart, render its employment objectionable and dangerous. Thallin, like kairin, reduces temperature by diminishing heat production, and by increasing heat radiation; as an antipyretic it is less dangerous, but no less objectionable, than kairin, for while its effect upon the ventricle of the heart is less depressing than that of kairin, its influence upon the blood-corpuscles is sufficient to condemn it. The action of hydrochinon is similar to that of kairin and thallin. Resorcin reduces the temperature by increasing heat radiation by the dilatation it produces in the capillaries and veins, especially the latter. Antipyrin reduces temperature purely by increasing heat radiation, owing to its extensively dilating the veins and capillaries; but what stamps it as an excellent antipyretic is that, besides dilating the veins, it also has a tonic influence on the heart and slightly increases arterial pressure, or, at any rate, does not cause a diminution of the same. It has, moreover, no

injurious influence on the blood or the muscular tissues, and strengthens the auricles. The objection to the employment of kairin and thallin as antipyretics arises from the fact that they cause heart paralysis, especially affecting the auricles, in doses only slightly larger than are sufficient to produce a lowering of the temperature. But this objection becomes an absolute danger when we take into account the destructive influence upon the blood-corpuscles and tissues generally. Hydrochinon and resorcin, although not exerting the same weakening and directly paralyzing influence upon the ventricle of the heart which is peculiar to kairin and thallin, both paralyze the venous side of the heart—viz., the auricles, and greatly lower the tone of the walls of the veins. The extra amount of blood, therefore, which is driven into the veins through the increased action of the ventricle, is only with great difficulty returned to the ventricle, and here the danger is not so much from failure in the power of the ventricle as in the case of kairin and thallin, as from the danger of *bleeding the animal to death into its own veins*. The intense visceral and especially pulmonary congestion found *post mortem*, by Dujardin-Beaumetz and others, in animals killed by resorcin, seems to confirm this view of the matter. Antipyrin, though largely dilating the veins, increases the power of contraction of both auricles and ventricle, and has no injurious influence upon the blood nor the muscular tissues, and therefore possesses, indeed, all the good qualities of a perfect antipyretic.

#### RIGHT-SIDED ENDOCARDITIS.

DR. BYROM BRAMWELL records, in the April number of *The American Journal of the Medical Sciences*, his experience with regard to the frequency of right-sided endocarditis. He finds (1) that right-sided endocarditis is much more frequent than is usually supposed; and that this conclusion is in no way contradicted, but on the contrary rather confirmed, by clinical evidence and clinical facts. (2) That Sibson's arguments against the tricuspid murmur of early acute rheumatism being indicative of right-sided endocarditis, are not valid. (3) That a tricuspid murmur occurring in the early stages of acute rheumatism in a previously healthy person who is not anæmic, is indicative of a rheumatic affection of the right heart. (4) That whether (a) the tricuspid regurgitation is the *direct* result of the inflammation of the tricuspid valve, or whether (b) it is due to a rheumatic affection of the wall of the right ventricle, with resulting relative or muscular incompetence, the pathological evidence seems to show that when the right heart is so affected in acute rheumatism as to produce a tricuspid leakage, inflammation of the endocardium of the right heart is often (usually?) present. (5) That although right-sided endocarditis is of frequent occurrence it is comparatively seldom followed by permanent organic disease of the tricuspid valve; in short, that right-sided endocarditis is an eminently curable affection. The importance

of this conclusion, if it be correct, can hardly be over-estimated. It is not a conclusion of mere scientific and pathological interest, but is of the greatest practical and therapeutical value. It shows the immense importance of rest in the treatment of endocarditis. The only reasonable explanation of the fact that mitral endocarditis is more severe and more frequently terminates in permanent valvular disease than tricuspid endocarditis, seems to be that the closure of the mitral segments is more forcible and that the inflamed mitral segments are subjected to greater strain than the tricuspid segments. In treating cases of mitral endocarditis our main objects should be to imitate Nature's method of cure; to place the mitral valve, so far as we are able to do so, in the same condition as the tricuspid valve—in other words, to reduce the force (and also the frequency) of the cardiac contractions and to allow the products of inflammation to be absorbed just as they are usually absorbed on the right side of the heart.

#### THE TREATMENT OF INTESTINAL OBSTRUCTION BY LAPAROTOMY.

DR. RANDOLPH WINSLOW reports, in the April number of *The American Journal of the Medical Sciences*, a case of acute intestinal obstruction successfully treated by laparotomy. In commenting on the treatment, Dr. Winslow expresses his opinion decidedly against any and all severe methods of attempting to overcome the obstruction, whether by rough manipulations or by rectal injections under strong pressure. On the other hand, he deprecates resorting to operation until a fair trial of medical means has failed to relieve the condition, and until it is reasonably certain that there is some mechanical hindrance to the passage of the fæces, which will terminate fatally unless relieved by operation. For obstruction in the large intestine, colotomy would in many cases afford relief. For persistent obstruction of the small intestine, his preference is decidedly in favour of laparotomy in the linea alba below the umbilicus, under rigid antiseptic precautions, as being the most precise, scientific, and rational means of discovering the cause and seat of trouble, and of remedying it at the same time. Whilst not underrating the risks of laparotomy, he does not think an exploratory incision to be a more serious procedure than abdominal taxis, or enemata under heavy pressure, and he thinks it much more certain and reliable in its results.

#### TREATMENT OF WHOOPING-COUGH.

DR. MONCORVO, of Rio de Janeiro, recommends pencilling of the isthmus of the fauces and laryngeal orifice with a 10 per cent. solution of hydrochlorate of cocain. After some minutes, the same parts are to be painted with a solution of resorcin, which he considers as the specific germicide of this disease.—*London Med. Record*, March 15.

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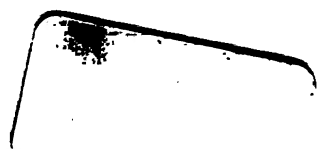
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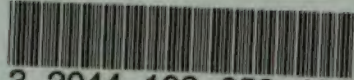
In Dr. H. C. Twedy's paper on "Cremation," in the May number, page 431, line 11 from top of page, for "ammonium" read "ammonia."

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